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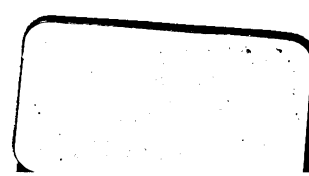

Newcastle-upon-Tyne Chemical Society.

TRANSACTIONS.

VOLUME I.

1868-71.

Newcastle-upon-Tyne:
A. REID, PRINTING COURT BUILDINGS, AKENSIDE HILL.
1871.

NEWCASTLE-UPON-TYNE:

A. REID, PRINTING COURT BUILDINGS, AKENSIDE HILL.

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OFFICERS.

1868-9.

President.

I. LOWTHIAN BELL.

Committee.

P. A. BERKLEY.
H. BOWMAN.
H. B. BRADY.
E. I. J. BROWELL.
R. C. CLAPHAM.
G. T. FRANCE.
J. GLOVER.
B. S. PROCTOR.
W. H. RICHARDSON.
J. W. SWAN.

Treasurer.

J. PATTINSON,
LABORATORY, *Side*, NEWCASTLE.

Secretaries.

GEO. LUNGE, PH.D., *Tyne Chemical Co.*, SOUTH SHIELDS.
A. FREIRE-MARRECO, *Neville Hall*, NEWCASTLE.

LIST OF MEMBERS.

APRIL, 1869.

		ELECTED.
1	Alder, T. S., Newcastle	July 29, 1868.
2	Allhusen, Alfred, Gateshead	,,
3	Armstrong, Sir W. G., K.C B., Elswick ...	,,
4	Allhusen, H. C., Gateshead	Dec. 23, 1868.
5	Adamson, L., Newcastle	Feb. 24, 1869.
6	Ayton, F., Hexham	,,
7	Ayton, W. G., Walker	,,
8	Aitchison, T., Walker	,,
9	Allen, Wm., Wallsend	March 25, 1869.
10	Austin, Edward, Southwick	,,
11	Berkley, P. A., Jarrow	July 29, 1868.
12	Bowron, James, Newcastle	,,
13	Bowron, W. M., Stockton	,,
14	Bowman, Henry, Washington	,,
15	Brady, H. B., Newcastle	,,
16	Brady, Alfred, Newcastle	,,
17	Browell, E. I. J., East Boldon	,,
18	Bell, I. Lowthian, Washington	,,
19	Bell, T. Hugh, Newcastle	,,
20	Black, William, Gateshead	Dec. 23, 1868.
21	Burnup, James M., Newcastle	,,
22	Bell, Thomas, Usworth	,,
23	Bowron, Joseph, Newcastle	,,
24	Bramwell, T. S., Heworth	,,
25	Bates, J., M.R.C.S., North Shields	Feb. 24, 1869.
26	Black, Joseph, Tynemouth	March 25, 1869.
27	Bainbridge, E. M., Seaham	April 22, 1869.

		Elected.
28	Clapham, R. Calvert, Walker	July 29, 1868.
29	Clapham, Henry, Newcastle	,,
30	Cail, William, Walker	,,
31	Chadwick, W., Hebburn	Dec. 23, 1868.
32	Chadwick, F. M., Hebburn	,,
33	Cook, T. Aynsley, Gateshead	,,
34	Cail, Richard, Walker	,,
35	Currie, Joseph, Sunderland	Feb. 24, 1869.
36	Douglas, R., Newcastle	July 29, 1868.
37	Daglish, John, Seaham	Dec. 23, 1868.
38	Elliott, Robert, Gateshead	July 29, 1868.
39	France, G. T., Gateshead	,,
40	Forster, John, Newcastle	Dec. 23, 1868.
41	Frank, J. M., Newcastle	,,
42	Glendinning, N., Felling	July 29, 1868.
43	Glover, John, Newcastle	,,
44	Gelsthorp, Charles, Jarrow	Dec. 23, 1868.
45	Gregson, T. L., Newcastle	,,
46	Gibb, Thomas, Jarrow	,,
47	Hardie, William, Newcastle	July 29, 1868.
48	Hansell, R., South Shields	,,
49	Hill, David, South Shields	,,
50	Holland, W. P., Jarrow	Dec. 23, 1868.
51	Jonassohn, M. J., Newcastle	,,
52	Johnson, Wm., Newcastle	March 25, 1869.
53	Little, Joseph, Jarrow	July 29, 1868.
54	Lunge, George, Ph.D., South Shields ...	,,
55	Leathart, James, Newcastle	Dec. 23, 1868.
56	Lomas, John, South Shields	,,
57	Lomas, Thomas, South Shields	,,

		ELECTED.
58	Lightfoot, T. B., Newcastle	March 25, 1869.
59	Mease, Geo. D., Jarrow	July 29, 1868.
60	Main, Edw. D., Gateshead	„
61	Moody, Wm., Washington	„
62	Moorhouse, T. F., South Shields	„
63	Marreco, A. Freire, Newcastle	„
64	M'Culloch, Jas., Gateshead	Dec. 23, 1868.
65	Noble, Capt., Newcastle	„
66	Napier, Jas., Hebburn	„
67	Noble, John, South Shields	„
68	Newall, R. S., Gateshead	„
69	O'Brien, W. C., M.D., South Shields	„
70	Proctor, W. W., Newcastle	July 29, 1868.
71	Proctor, B. S., Newcastle	„
72	Pattinson, John, Newcastle	„
73	Pattinson, W. W., Felling	Dec. 23, 1868.
74	Palmer, C. M., Jarrow	„
75	Richardson, W. H., Jarrow	July 29, 1868.
76	Reid, W. C., Blaydon	„
77	Rennoldson, Wm., Jarrow	„
78	Richardson, J. Hancock, Gateshead	Dec. 23, 1868.
79	Russell, Wm., Wallsend	„
80	Redmayne, R. R., Felling	Feb. 24, 1869.
81	Swan, J. W., Newcastle	July 29, 1868.
82	Scholefield, H., Newcastle	„
83	Schwarz, H., M.D., North Shields	„
84	Stead, John, Newcastle	Dec. 23, 1868.
85	Stuart, F. W., Gateshead	„
86	Spencer, John, Newburn	„
87	Speir, Jas., Newcastle	„
88	Stevenson, J. C., M.P., South Shields ...	„

		ELECTED.
89	Stevenson, Arch., South Shields	Dec. 23, 1868.
90	Swan, J. Cameron, Newcastle	Feb. 24, 1869.
91	Stevenson, Alex. S., Tynemouth	March 25, 1869.
92	Taylor, John, Earsdon	Dec. 23, 1868.
93	Thomas, John T., Walker	March 25, 1869.
94	Vasey, T. E., Jarrow	July 29, 1868.
95	Warwick, John, Wallsend	April 22, 1869.
96	Wilson, A. W., Ulverstone	July 29, 1868.

The Corresponding Secretary will be obliged by notice of changes of residence, addressed to him at Neville Hall, Newcastle.

RULES.

1.—This Society shall be called "The Newcastle Chemical Society."

2.—The objects of this Society shall be the promotion of Chemistry and the allied sciences in this district, more especially with regard to their practical application. The reading and discussion of papers; the exhibition of apparatus and specimens; and, if found desirable and practicable, the purchase of apparatus and books, to be the means to this end.

3.—Intending Members shall be nominated by not less than two Members of the Society at one meeting, and balloted for at the next; one-third of the number of Members present in black balls shall exclude.

4.—The Annual Subscription shall be Ten Shillings, due on election, and thereafter at the Annual Meeting. Members whose subscriptions shall be twelve months in arrear, shall, after notice, be removed from the list.

5.—The Meetings shall be held monthly, from October to March inclusive, and at such other times as the Committee shall determine. The first Meeting in October shall be considered the Annual Meeting.

6.—The Officers shall be elected at the Annual Meeting, and shall consist of President, Treasurer, two Secretaries, and a Committee of ten. Any Member may nominate any eligible person or persons (not exceeding the required number) to fill up the respective offices, and voting papers shall be supplied to Members ten days previous to the Annual General Meeting, upon which Members shall write the names of the Members they propose as Officers, and deliver the same to the Secretaries, or at the Meeting. The Chairman shall appoint scrutineers, who shall examine the lists.

7.—Any Member proposing to read a paper, shall forward the same, or a summary of its contents, to the Secretaries, for the approval of the Committee; but this rule shall not preclude Members from bringing forward subjects of interest for discussion.

8.—The Committee shall have power to apply the funds of the Society in such manner as they shall consider best adapted to promote its objects, in accordance with Rule 2. A printed account of receipts and disbursements shall be presented to the Annual Meeting.

9.—Any Member proposing to alter any of the above rules, shall give notice of his motion in writing to the Secretaries, not less than one month previous to the Annual Meeting. A copy of the proposed alteration shall be sent to each Member.

10.—At any ordinary Meeting any Member shall be allowed to introduce friends who may read papers, or take part in the discussion.

Newcastle-upon-Tyne Chemical Society.

FIRST GENERAL MEETING, DECEMBER 23RD, 1868.

I. LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

Thirty-eight new members were elected. Mr. P. A. Berkley was elected on the Committee in place of Mr. John Pattinson appointed Treasurer.

On the motion of the PRESIDENT it was resolved:—"That the ordinary meetings of the Society be held on the fourth Thursday in each month."

The PRESIDENT then delivered the following

INAUGURAL ADDRESS.

GENTLEMEN,—Before I enter upon the proper duty of an address, by means of which it is intended to introduce your society to public notice, in the hope of obtaining for it a due share of public recognition and support, permit me to tender my very grateful acknowledgments for the honour which the selection to serve as your first President necessarily confers. Surrounded as I am by gentlemen possessing high attainments in the science which it will be the object of this body to cultivate, I feel how entirely I am indebted to your kindness, in your being willing to accept as a sufficient qualification for the post the unequivocal interest I have invariably manifested in the application of chemistry to art. This claim, such as it is, I shall endeavour to supplement by an assiduous attention to the duties of my office, in the expectation that thereby, and with your assistance, I may be enabled to render substantial service to our progress and success. Gentlemen, the banks of the Tyne are not without a certain degree of historical

interest in connection with the development of chemistry applied on a large scale in various manufacturing operations of great importance and extent. More than seventy years ago, the late William Losh, in company with Lord Dundonald, commenced his labours to find a substitute for potashes, then exclusively employed whenever pure alkali was required in the arts. These pioneers in the manufacture of soda endeavoured in succession to accomplish their object by decomposing common salt by means of oxide of lead, by sulphate of iron, by carbonate of potash, and finally by the adoption of Leblanc's process of decomposing salt by sulphuric acid, and then subjecting the sulphate of soda to the joint action of coal and lime—the process in fact of the present day. Of still more ancient date is the commencement of the production of prussiate of potash, which was introduced into this neighbourhood about the beginning of last century, soon after its discovery by Diesbach, in Berlin. Close on a hundred years ago, soap was established as one of the manufactures of our district, and for a still longer period have the coal mines afforded to the copperas makers the raw materials required in their process. Glass, although not usually regarded as a chemical product, is in the eyes of a chemist strictly one, and it is, therefore, worthy of note that its manufacture was begun on the banks of the Tyne so far back as 1615. But if the antiquity of the importation of chemical operations into this vicinity presents any features of interest, how greatly must that interest be magnified when we contemplate the expansion which has marked the career of our manufacturing chemists during the last five-and-twenty years. Immense establishments have sprung up to supply the ever-increasing demand of that alkali produced by Mr. Losh in hundredweights, while others have multiplied for obtaining bleaching powder, bicarbonate of soda, various combinations of lead, glass, earthenware, which, with our lead, copper, iron, and many other works, provide employment for thousands of men, and occupation for numerous practical chemists. Under these circumstances, and looking at the fact that we have among us professional chemists of great reputation, as well as many practical men who take a pleasure in study, it certainly is somewhat remarkable that it has been reserved for the present day to witness the inauguration of the first Chemical Society in the North of England. The absence of such an organization is rendered still more conspicuous by the

continual appeals made to chemistry itself, not only in our manufactories, but also in a vast number of commercial transactions, which, until recently, were conducted in a somewhat empirical mode. In lending my humble assistance to the furtherance of the objects of inquiry which it will be the aim of this Association to promote, I do so in the belief that not only shall we be instrumental, by the opportunities our meetings and discussions will afford, of adding to each other's knowledge upon matters more directly connected with our various occupations, but we shall also have the means, by carefully observing phenomena passing daily before us, of adding something to that store of information which has rendered chemistry one of the most attractive and comprehensive sciences of the day. With the constant claims upon your attention, and with the anxieties inseparable from the superintendence of large works, it may appear that I am taking a somewhat sanguine view of the results which I anticipate may flow from the establishment of this society, and that it is scarcely to be expected that we as manufacturers can add much, or anything, to a subject which is occupying the exclusive attention of so many illustrious chemists at the present time. To those who are disposed to take this view of our future, I would remark that the opportunities we possess of observation differ very materially in some respects from those usually within the reach of the chemical philosopher, whose operations are for the most part carried on for a comparatively brief space of time, and upon very minute quantities. You, on the other hand, have in addition to the means afforded by your laboratories, a field of inquiry and research within your power rarely or never at the command of the man of science. His short experiment is frequently substituted in your case by prolonged action, extending over weeks, it may be years, while the ounce of material under treatment may be magnified to hundreds of tons. To him who feels an interest in observing nature and in seeking to account for effects daily meeting the eye, occasions are continually presenting themselves of comparing agencies at work on a miniature scale with those which probably served to build up our strata, or with others which subsequently thrust up the sedimentary rocks into our loftiest mountain ranges. Currents of water leaving our manufactories, laden with flocculent matter, often give rise to substances resembling in character

the aqueous deposited portions of the crust of our earth, while our flowing slags, charged with compressed air, spurt up from their tiny craters pigmy torrents of lava, which on slowly cooling afford many of the leading features of our crystalline rocks. At other times you have the linings of our blast furnaces exposed to years of intense heat, imitating, in some measure, the columnar appearance of basaltic pillars, or the mechanical structure of some of our ores reproduced by the gradual deposition of carbon from the volatile contents of our coke ovens. Cut through an old spoil heap, which has undergone slow combustion, and the chances are you will come on some cavity lined with crystals, sublimed from the chaotic mass, from which you may gather some information as to the probable cause of the occurrence of shining ore or sparkling gem in the crevices connected with the vast alembics of nature. Only two years ago our townsman, Mr. Henry B. Brady, discovered in the insoluble matter left by treating limestone with hydrochloric acid at one of our chemical works, siliceous casts of the calcareous skeletons of half-a-dozen carboniferous limestone corals, and among them are species which up to that time had not been previously described. Thus, independently of the means we possess of studying the effects mass and time have in modifying and altering many of the results as they are obtained in the laboratory, it appears to me the connecting of our daily operations with such observations as those to which I have been alluding, would confer an interest upon much which would otherwise be dry and perhaps uninstructive routine. Davy defines the foundations of chemical philosophy to consist in observation, experiment, and analogy. By observation, facts are distinctly and minutely impressed on the mind. By analogy, similar facts are connected. By experiment, new facts are discovered; and in the progression of knowledge, observation, guided by analogy, leads to experiment, and analogy, confirmed by experiment, becomes scientific truth. Thus the very ground-work of the whole superstructure is observation; it must not, however, be overlooked that the power of noting a new fact is not a common one—it is a faculty requiring for its development much and careful cultivation; hence no opportunity ought to be lost of improving ourselves in the habit of observing, for, as Humboldt truly says, we must never forget that in the observation of a phenomenon, which at first sight appears to

be wholly isolated, may be concealed the germ of an important discovery. Applying this language to circumstances within our own knowledge as practical chemists, have we not Neilson perceiving that the fire in a smith's hearth had its temperature raised by heating the blast in an iron tube previous to its arrival at the burning coal? and have we not our late townsman, Hugh Lee Pattinson, observing that lead in cooling solidified unequally? Yet these two simple facts embody two great philosophical truths, upon which have been founded two of the greatest improvements in metallurgical science of modern times, viz., the hot blast applied to our iron furnaces, and the Pattinson process of desilverizing lead. As instances of the advantages possessed by manufacturers for promoting chemical science, I may quote the discovery of selenium and thallium, found in infinitesimal quantities in the slow accumulations of sublimate in our vitriol chambers; while bromine as well as iodine were first noticed in the concentrated mother waters obtained in treating sea water and in operating on the incinerated portions of marine plants. The affinity, as you are aware, which binds carbon and nitrogen together to form cyanogen is so feeble, that the direct combination of these two elements in the requisite proportions is not an easy matter. The problem was attempted to be solved practically by the late Mr. Thomas Bramwell, who, in order to obtain prussiate of potash, passed nitrogen up a column filled with incandescent charcoal previously saturated with a solution of the alkali. According to information given me by Mr. Bramwell and his son, the failure, commercially speaking, was entirely due to the difficulties attending the maintenance of the apparatus; otherwise, the experience gained by that gentleman entirely confirmed the lesson first taught by our blast furnaces, from which cyanide of potassium is occasionally gathered in considerable quantities. More recently I had occasion to reduce to about 80 deg. F. the temperature of the top of one of the Clarence iron furnaces by the use of raw instead of calcined ironstone. The expelled water condensed in the gas tubes, and ran down, forming long stalactites of chloride of ammonium. As coke alone was the fuel employed, is it possible that the hygrometric moisture afforded the hydrogen which united with the nitrogen of the blast? and the alkali thus generated was seized upon by hydrochloric acid expelled at a

high temperature from the chloride of sodium, always present in our coke. Now, although we ought, as members of this chemical society, ever to keep in view the duty of endeavouring to cultivate a taste for chemistry for its own sake, we cannot shut our eyes to the fact that much of our popularity, and indeed, I may say, much of our usefulness, will be dependent upon the circumstance of our connection with those branches of manufacture which require, for their successful management, some chemical knowledge. It would, therefore, be a source of great satisfaction to me if any remarks I may make could raise in your minds the importance and dignity of your industrial pursuits, and at the same time enlarge any estimate you may have formed of the high value you ought, as manufacturers, to attach to science as the proper ground work of all your operations. I do not think I can quote a higher authority than the writer of "Cosmos," himself, a philosopher as well as a statesman, in support of the pretensions of industry, as well as of the necessity of its alliance with a knowledge of those laws of matter made known to us by scientific research. "Those states," he observes, "which take no active part in the general industrial movement, in the choice and preparation of natural substances, or in the application of mechanics and chemistry, and among whom this activity is not appreciated by all classes of society, will infallibly see their prosperity diminish in proportion as neighbouring countries become strengthened and invigorated under the genial influence of arts and sciences. As in nobler spheres of thought and sentiment, in philosophy, poetry, and the fine arts, the object at which we aim ought to be an inward one—an ennoblement of the intellect—so ought we likewise, in our pursuit of science, to strive after a knowledge of the laws and principles of unity that pervade the vital forces of the universe; and it is by such a course that physical studies may be made subservient to the progress of industry, which is a conquest of mind over matter. By a happy connection of causes and effects, we often see the useful linked to the beautiful and the exalted. The improvement of agriculture in the hands of free men and on properties of a moderate extent—the flourishing state of the mechanical arts freed from the trammels of municipal restriction—the increased impetus imparted to commerce by the multiplied means of contact of nations with each other—are

all brilliant results of the intellectual progress of mankind, and of the amelioration of political institutions in which this progress is reflected." In these words the Prussian philosopher has not only completely covered the ground I referred to him to sustain, but he imposes on you the additional obligation of being the instruments of promoting, in proportion to your advancement and success, the civilisation of the human race, not alone of that civilisation which marks the transition from a state of mere barbarism, but one which more effectually than the force of arms, and more permanently than the language of diplomatists, will secure for all nations, on earth peace and goodwill towards men. With Humboldt's counsel fresh in our minds, recommending this union of science with art, one is ready to contemplate with pleasure the instances in which our scientific colleagues by their labours have lightened our own, or by entering upon new fields of inquiry have stamped their researches with additional value by connecting their discoveries with the common requirements and purposes of life. This undoubtedly was Davy's idea when his masterly researches on flame ended in the invention of the safety-lamp, for he felt he was promoting the cause of science by proving that apparently abstract philosophical truths may be beneficially applied to the ordinary wants of mankind. Probably an additional charm would have spread itself over the labours of Wenzel, Richter, Berzelius, and Dalton, had they been able to foresee what daily application in our calculations would have followed their elaboration of the laws of chemical equivalents, for by these laws we have been enabled, by a simple series of numbers, to grasp all the figures representing every conceivable order of chemical combination, and by the results we are at once placed in a position to ascertain the extreme limits of produce from the materials under treatment. Let your imagination carry you back into the laboratory of Lord Dundonald and Mr. Losh at the time they were engaged in their experiments in the manufacture of soda, and then compare their position with your own. In those days almost every fact was required to be ascertained by those needing the information, and this had to be worked out with imperfect apparatus, assisted by still more imperfect doctrine, for such, no doubt, you will consider the opinion of Fourcroy, one of the best and most compendious chemical writers of his period. In his system of chemistry, printed

in 1804, in 11 volumes, he mentions "that some facts and analogies had induced him to think for several years past that soda" (the very subject of Mr. Losh's inquiry) "was magnesia saturated with azote." It is only due, however, to the Frenchman to state that he gave this merely as a hypothesis, which ought not to be confounded with the exact acquisitions of science. At present, there is scarcely a fact connected with either the physical or chemical properties of every well-known body, not duly recorded in those excellent treatises of chemistry, at the command of every one. Our apparatus is simple and inexpensive, at least for ordinary purposes, and the methods of analysis devised by scientific men are remarkable, generally speaking, for the correctness of the results they afford. I am not going to attempt to render any homage to the extensive labours of those philosophers who, in the last 25 years, have added so immensely to organic chemistry; but I cannot omit an opportunity of alluding, in passing, to the wonderful success which has accompanied their researches into the substances derived from coal-tar, or rather from one of its products, viz., benzol. From this, as you are aware, dyes of almost every shade of colour, of the most exquisite and brilliant hues, have been produced by the various salts of aniline obtained from this source. While on the subject of applied chemistry, I may be allowed to refer to the now highly-cultivated art of photography, every step in which requires the aid of chemical action. This allusion enables me to call your attention to the very ingenious method of carbon printing brought to such a state of perfection by our townsman, Mr. J. W. Swan, as to have raised it to the position of a distinct branch of industry. In the ordinary practice of photography a picture is taken, generally on glass, by exposing a salt of silver in a camera to the action of light. This negative impression is reversed by printing from it by transmitted light others on paper previously immersed in a solution of silver. The usual photographs, therefore, are films of a silver salt acted on by light, the clearness of which, however, is liable to be impaired by time and exposure—an objection entirely removed by receiving the picture from the glass negative on gelatine instead of on silver prepared paper. This elegant operation, as many of you probably are aware, depends on the fact that gelatine is rendered insoluble in water by the action of bichromate of potash

when submitted to the influence of light. A mixture, then, of these two substances in the form of a thin film, on being exposed to the solar rays, has more or less of its thickness converted into an insoluble substance, according to the intensity of the light thrown upon it, leaving the remainder to be washed off by suitable means. You have thus a film of gelatine varying in thickness in the ratio of the varying intensity of light transmitted to its surface through the negative picture on glass. By incorporating in the substance of the gelatine carbon, or other suitable pigment, you have an impression in which the depth of the shade increases with the thickness of the film. Processes such as this afford striking examples of the application of principles without a knowledge of which no approximation to the results obtained could ever have been looked for. True it is, however, that instances are by no means wanting of considerable progress having been made in the arts in the absence of all chemical knowledge, or, indeed, of any attempt to generalise the experience gained in one branch of manufacture with that acquired in another. Progress under such circumstances was necessarily slow, where much would be left to chance, for what but chance could have induced our ironmasters of former days to add the infusible earth lime as a flux to their ores in the absence of all information on the properties of the double silicates? Again, we owe the introduction of the preparation of alum to an ancestor of my friend, Admiral Chaloner, of Guisbro', imagining he traced some resemblance between the shale of the Cleveland Hills and the raw material used in the Roman States for making this salt. But for this accident, and we might have continued to import all our alum from Italy until chemistry had revealed it to be a sulphate of alumina, and that we possessed inexhaustible supplies of its constituent parts under our very feet. But it is when we are brought face to face with any real difficulty that we recognise the true value of science by the readiness with which she affords succour when appealed to for assistance. Sulphuric acid, distilled from green vitriol, was known to the alchemists of old, but it was long after their time before mankind learnt to produce the same substance by the combustion of sulphur and the use of nitrate of potash. In 1839, a political blunder in Naples doubled the price of sulphur in this country; but geology had taught us where to look for pyrites, and chemistry had shown it to be a sul-

phuret of iron capable of yielding sulphur by its own combustion. Hence, in the following year, we were independent of the caprice of the Bourbon King for the means of obtaining that acid, which to lose would fall not short of a national calamity. More recently certain fiscal changes threatened embarrassment to our paper-makers in obtaining their raw material—rags. Our neighbour on the Wear, Mr. Thomas Routledge, knew something of the existence of a weed growing on the sandy coasts of Spain, and he was not ignorant of the power caustic soda possessed in removing the silica and resin abounding in this vegetable substance, and to-day, after a few short years, I dare say there is not, certainly there need not be, a newspaper printed in Newcastle which is not, so far as the paper is concerned, entirely procured from Esparto grass. A considerable time might be devoted to an interesting enumeration of the various examples of the application to useful purposes of substances previously known, of others new to us, or of waste and consequently worthless products, for which we are chiefly, and in many cases entirely, indebted to the labours of scientific men, who have made certain branches of study the object of special inquiry. Cheuvreul's experiments, as an instance, first made us acquainted with the nature of fatty matters. From him we learnt they were composed of an acid and a base, in short, chemically speaking, all fats or oils are salts containing stearic acid and glycerine as the base. His investigations, followed up by practical men, with science helping them on their way, have enabled us to separate these two elements with facility, in fact by simple distillation, at a very high temperature; and the tallow candle of our younger days is replaced by the elegant and economical one of stearine, while the glycerine is applied to a great variety of uses, including that of the manufacture of that highly-explosive substance nitro-glycerine. Among the triumphs of modern chemistry may be quoted those excellent modes of analysis which have enabled us to detect and appreciate extremely minute portions of matter existing in combination with other substances. It is obvious, however, that there is a limit beyond which our senses cannot carry us; for, arrived at a given point, the human eye fails to detect a precipitate, or distinguish any change of colour. I know of no plan, nor am I aware of any facts known to chemists, which would have led them to suspect the presence of silver in sea

water; yet there now seems no doubt that copper, after being exposed for a long time on the bottoms of ships, indicates the existence of silver where none was to be found before, a circumstance which can only be accounted for by supposing that there is in the waters of the ocean an infinitesimal quantity of this metal, which goes on accumulating upon the sheathing of vessels until its quantity reaches a point susceptible of being detected. It is obvious, however, that the circumstances are rare where the existence of a substance can be made perceptible to our senses in the manner just described in the case of silver in sea water. That which, however, was impossible by any of the ordinary methods known to chemists, has been placed within our grasp by Kirchhoff's researches into the nature of the dark lines in the spectrum. The sequence of discovery which has led up to the development of so-called spectrum analysis constitutes one of the most interesting records in the history and progress of physical science. Two hundred years ago, Newton demonstrated the compound nature of white light by resolving it, by means of the prism, into the exquisite tints of the rainbow. The component parts of the spectrum of light, 150 years after Newton's time, were observed by Wollaston and Fraunhofer not to be continuous, but that its colours were interrupted by a considerable number of dark lines. Less than ten years ago Kirchhoff first made known the true character of these lines. Excepting for the powers possessed by this extraordinary discovery of this philosopher of indicating the presence of inconceivably minute traces of substances and proclaiming the fact in large characters before our eyes, it is not too much to say that in all probability we should have remained for ever ignorant of the existence of matter in certain new forms, or of its presence in a position otherwise inaccessible. By the instrumentality of the spectrum analysis we have had revealed to us the new metal thallium, cœsium, and others, and, to use the words of Mr. Huggins, "in the dark lines of the spectrum are the symbols in which is written the chemical constitution of the sun." This is not the language of hyperbole, for the labours of this last-named gentleman, assisted by Dr. W. Allen Miller, may be said to have annihilated the 95 millions of miles of space which separate us from that luminary, and to have demonstrated almost as conclusively as if a portion of his atmosphere had been transported within the walls of a

laboratory, the nature of a part at least of its constituent elements. But this affords but a faint idea of the limits of those distances at which the existence of substances familiarly known to ourselves may be considered as now proved. Sodium, magnesium, and iron have been detected in stars, whose remoteness is not, cannot be, reckoned by millions of miles, but by the hundreds of years light travelling at the rate of 190,000 miles a second would take to reach our planet; nay, those patches of phosphorescent haze known as nebulæ, have already to some extent yielded up the secret of their composition by the indication of the nitrogen and hydrogen lines in the spectrum. It would take me far beyond the time to which I must confine my address were I to detain you at any length on the subject of spectrum analysis. I shall, therefore, content myself by reminding you of the three orders of spectra. You have first the coloured band, unbroken by either dark or bright lines. Such a spectrum would be emitted by an incandescent and opaque body, either in the condition of a liquid or solid. The charcoal points of the electric light, heated iron, or any other light-emitting substance, afford this spectrum without any indication being given of the chemical nature of the incandescent body. The second order of spectra consists of coloured lines of light separated from each other, the colours themselves being produced by the luminous matter being in the gaseous form; but these coloured lines vary according to the character of the luminous vapour from which they proceed. Introduce, for example, between the charcoal points of the electric lamp, the metal zinc, and you have a purple light emitted by its vapour. In like manner, iron will give you red lines, sodium yellow lines, copper green lines, and so on. Spectra of the third order are produced when the light emitted, as in the case of spectra of the first order, passes on its way through certain vapours not of themselves luminous. These spectra are distinguished by their coloured lines being interrupted by dark lines such as those observed by Wollaston and Fraunhofer in the prismatic colours formed by the decomposition of the solar ray. Now, Kirchhoff's grand discovery consisted in showing that the position of a group of dark lines in the spectrum, produced by the light passing through vapours of terrestrial substances, coincides exactly with that of the luminous bands afforded by the light of the vapour of the same substances when rendered

luminous. For example, luminous silver vapour gives certain green lines in the spectrum, but if light proceeding from a highly incandescent solid or liquid body passed through an atmosphere of silver vapour, the vapour itself not being luminous, then dark lines would be visible in the spectrum, agreeing perfectly in position with those bands of green light produced by the luminous vapour of silver. It is then by means of the position of these dark lines produced in the spectra by the light emanating from the sun, the stars, the nebulæ, and comets, that Mr. Huggins has propounded to the world the probable composition of the atmosphere of these heavenly bodies. Upon one occasion, mentioned in an intensely interesting lecture by Mr. Huggins, I heard at the Nottingham meeting of the British Association, a star in the constellation Corona increased suddenly in brightness, which continued for some time. From the period of its greatest luminosity it declined in twelve days from a star of the second magnitude down to one of the eighth, and the lines produced during the time that this body continued more conspicuous than usual, suggested, as the lecturer truly called it, "the startling speculation that the star itself had suddenly become enrapt in the flame of burning hydrogen produced by some great convulsion." This identification of matter as it exists in the remotest confines of space from which light reaches the earth, with the forms under which it is familiarly known to ourselves, opens out a wonderful vista for the future of philosophical inquiry, for it is not unreasonable to suppose that those phenomena which we witness at even immeasurable distances, may be evolved by the same causes producing similar effects upon matter within the scope of our immediate observation. The experiments of Joule have demonstrated that a pound weight falling from a height of 772 feet has motion which, when represented by its equivalent of heat, means that it is equal to heating one pound of water one degree of Fahrenheit. Admitting the correctness of the hypothesis that the high temperature of the sun is maintained without loss by meteoric matter being drawn to its surface, it would be competent for us to estimate the quantity of, say iron, and the height from which it would have to fall, to produce an amount of heat capable of raising every hour 700,000 million cubic miles of water from the freezing to the boiling point, for such represents, it has been estimated,

the amount of heat constantly emitted by our central luminary. I believe when a spectrum is produced by a flame from an ordinary lamp the sodium lines are never absent, and this circumstance has been accounted for by the supposed constant presence of soda, in some or other of its combinations, in our atmosphere. This idea has been confirmed by some highly instructive experiments of M. Desiré Gernez. You will remember that at a temperature of 91° F. water dissolves about half its own weight of anhydrous sulphate of soda, whereas at 60° F. it requires six of water to dissolve one of this salt: in other words, that 100 parts of water at 91° saturated with sulphate of soda will deposit, on cooling to 60°, just about one-third of its weight of this substance reckoned in its anhydrous condition. Instead, however, of this sulphate following the ordinary rule of solution, *i.e.*, of depositing in the form of crystals the difference of the quantity capable of being dissolved at one temperature and another, you are aware, by excluding the air, a solution of sulphate of soda made at the maximum point of solubility, viz., 91°, may be cooled down to 60° and lower without crystallizing. On the admission of air, however, generally speaking, instantaneous crystallization takes place. This was ascribed by Gay Lussac to the concussion of the air upon the surface of the liquid. At the same time that distinguished chemist was perfectly acquainted with the fact that a little oil or even a sheet of paper interposed between the solution and the atmosphere invariably, in spite of the influence of concussion, prevented crystallization. In 1815, Ziz discovered that any substance, however small, such as a needle, previously exposed to the atmosphere, always induced the change; but that, if this needle had been previously wetted or heated, crystallization did not ensue on contact being made between it and the solution. Now, M. Gernez has proved by the experiments I have referred to, that the crystallization under the circumstances described, is due to the fact that floating in the air, even far from the sea and from large towns, there is constantly present in very minute quantities sulphate of soda. Admit it to the supersaturated solution, and crystallization ensues; exclude it, and no crystallization takes place. These researches in themselves are so interesting that I cannot resist quoting one or two. Suppose a quantity of the supersaturated solution to be contained in a glass globe with a moderately long neck, air is admitted and crystal-

lization commences from a point immediately below the opening; that is, in the centre of the liquid, as if something falling down the orifice had induced the change. In proof of the correctness of this theory may be adduced the circumstance, that if the glass vessel be kept inclined so that nothing can fall down its neck, no crystallization occurs; and again, if the neck be so bent as to expose its open end upright, by which arrangement anything falling into the orifice will be retained at the bend, there is no crystallization, but raise the vessel up, so that any substance caught at the bend may fall into the liquid, and you have immediate crystallization. M. Gernez subsequently, by the continued aspiration of a large quantity of air through water, actually succeeded in obtaining crystals of sulphate of soda. I have dwelt at greater length upon these investigations of this French chemist than I otherwise should have done, because it may lead to some practical results of importance. Gernez observed that when the air, which was admitted into contact with the solution, was made to pass through cotton, the particles of atmospheric sulphate were filtered, as it were, from it, and no crystallization ensued; but that this cotton, now holding in its substance the imprisoned salt, caused the solidification when brought in contact with the solution. Now, in 1858, something like the same conclusion was come to by Schrœder, in respect to fermentation and putrefaction. His experiments, if I remember correctly, established the fact that if the air necessary to induce either of these changes was filtered in like manner through cotton no alteration was produced, but that the cotton itself took up from the air certain miasms which rendered it capable of producing either the one or the other on being immersed in the substance in which it was desired to excite fermentation. The question is whether air, laden with those principles which are supposed to provoke cholera, fevers, and so forth, might not be treated in a similar manner, and, by examining the nature of the deposit, some mode of treatment, founded on philosophical induction, be devised for averting those terrible calamities the causes of which are now hidden in so much mystery. Only very recently, the present Master of the Mint, Dr. Graham, has given to the world a series of highly interesting investigations, on what he terms the absorption and dialytic separation of gases by colloid septa. His experiments have been followed

by the researches of H. St. Claire Deville, who has discovered the surprising facility with which gaseous substances are transmitted through the homogeneous substance of iron or platinum at a red heat; and Dr. Graham himself has ascertained that the comparatively dense metal palladium is capable of absorbing, at about 212° F., 643 times its own volume of hydrogen gas. My object in alluding to these facts is to have an opportunity of quoting the application of endosmosis as a means of detecting fire-damp in our mines. In an ingenious form of apparatus, devised by Mr. Ansell, the passage of a portion of the carburetted hydrogen through a diaphragm of earthenware or marble alters the level of a portion of mercury, and thus, by completing a galvanic circuit, warns the miner of his danger by ringing a bell. The important offices oxygen performs in the economy of nature, as well as in your own workshops, invests all its properties with no ordinary interest. Its capability of having its chemical agency intensified, by being made to assume the form of ozone, necessarily, therefore, commands our attention. Whether this property can ever be rendered available in the arts must depend on the facility with which the change can be effected. Mr. Ladd has brought with him a piece of ingeniously-devised apparatus for the conversion of the oxygen of the air into ozone, which will afford, to those interested in the question, an opportunity of witnessing the operation. This mysterious condition of oxygen courts, and no doubt will reward, further inquiry, as indeed do many other questions now engaging, for the first time, the research of scientific men. Among them, I would name the beautiful phenomena attending the passage of the electric fluid through a partial vacuum, at present one of those germs alluded to by a philosopher already quoted as possibly concealing some great discovery, it may be, of not less importance than the electric telegraph developed by means of an observation made twenty years previously, that a magnetic needle was deflected at right angles by the passage of an electric current over it. For the further extension of this sublime application of philosophical truth to the wants of mankind, so that not even the depths of the ocean can interfere with the communication of thought between nations, we are indebted to the present Mayor of Gateshead, Mr. Newall, who, with his own hands, insulated the

conducting copper by means of gutta percha, protected the whole by his wire rope, and thus produced the first submarine cable. Entering once again within the walls of your manufactories, allow me to remind you, as men entrusted with the discharge of duties so eloquently eulogised by the great Humboldt, that there, awaiting solution, are some questions of, I may say, national importance. You are invited to rid our iron of impurities seriously impairing its properties, one, at least, of which, having a great value taken separately, is worse than useless when combined with iron. You have our iron slags, rich in metal, but nearly worthless, from the accumulation of phosphorus, so much sought for elsewhere, which they contain. All the sulphur consumed in your alkali works, after serving its purpose of decomposing your common salt, is ungratefully expelled from your premises, and permitted to accumulate in vast heaps to the annoyance of your neighbours. There are many others which time, however, forbids my even alluding to; but I cannot close the list without a few remarks on one material common to almost every species of manufacturing industry—I mean the coal we require in our various mechanical and chemical processes. During the last year or two, the subject of our fuel has received an impetus in public estimation by the calculations which have been made to ascertain the probable date at which, from absolute exhaustion of our coal beds, or from the greatly enhanced value of their produce, our furnaces will be extinguished, and our position as a nation a matter of history. With these calculations we shall not trouble ourselves, open as they are to so many disturbing influences. My duty will rather be confined to urge on you that attention to those precautions being observed in the use of coal which may promote economy, and, at the same time, interfere as little as possible with public convenience and comfort. Economy in the use of an article, such as that we are considering, necessarily means, in the first instance, gain to the consumer. Shut out all hope of this, and no alarm for the future will alter his course of conduct. Let me illustrate this by actual experience. The annual make of pig-iron in our district is such that twenty years ago, five millions of tons of coal would have been required for its production. At present, something like two and a half millions will suffice for the work, but to effect this saving our coke-burners and iron-smelters have expended considerably above

£500,000. I need scarcely say fears for the position of posterity have formed no element in their calculations when they undertook the outlay of so large a sum. In this branch of our inquiry, as in many others already mentioned, we have received substantial assistance from the labours of science. In former years, long experience and rule of thumb trials might and did lead to improvements in the construction of our furnaces; but the manufacturer was without any data by which he could estimate the absolute power of his fuel and compare it with the duty performed. Your position now is essentially different. We know the units of heat developed by burning the coke in our blast-furnaces, and we set against that the quantity chemistry has shown to be required for reducing and melting our iron, the loss from the escaping gases, and the cooling of our metal and slag, and we study the margin for further improvement. Although considerable amelioration, in many instances, has been latterly effected in the application of heat, there still remains much to be done. To what extent have we, for example, satisfied ourselves by actual analysis of the character of the combustion effected in our ordinary furnaces? That this is the very reverse of perfect is obvious, without any appeal to the laboratory, for the immense clouds of smoke, under which we are condemned to live, proclaim too surely the great waste which is constantly going on at our very doors. I would have the members of the Chemical Society to take up this subject in good earnest, not by the indiscriminate employment of the numerous schemes courting public approval, but by a patient and philosophical examination of the different furnaces under your care. That the question is by no means a simple one, is obvious, from a moment's reflection on the nature of the coal you employ. For every 100 lbs. of it you burn, in practice, more than 2,000 lbs. of atmospheric air pass over or through your fire-place. Of the portion actually consumed by the fuel, about two-thirds only are taken up by the solid carbon, which represents the uniform action of the furnace. The remaining third of the air, entering into combination, is required by the volatile portions of the coal, and their emission, as we all know, is accompanied with excessive irregularity, hence the smoke which mars the beauty of our landscape, defaces our buildings, and is a source of discomfort and annoyance to every one exposed to it. Its continuance

to the extent we daily witness is a disgrace to the intelligence of the age. Again, how seldom do we examine the loss from escape up our chimneys? From many of our steam boiler furnaces you will find the temperature of the flues to be 800° to near 1,000°; although Peclet has shown in his Treatise on Heat that, at something like 570° the maximum effect of the exhausting power of a chimney is obtained. Now, although I have refrained from examining with you any of the calculations connected with the duration of our coal-fields, it is obvious that no questions connected with the probable extent of supply of fuel can be devoid of interest to a community such as ours; particularly when they bear immediately on our own district. We have been recently informed from the Northern Mining Institute that in all likelihood we may hope to reach and work coal at a distance of 12 miles from our shores by sinking shafts in the open sea. I need scarcely point out to you the great importance of this addition to our resources, although we are not informed upon what grounds the calculations are based, which prolong our coal seams beneath the ocean to this extent. This, of course, is not the place, nor am I the person to speak with any authority on the practicability of this bold step in mining engineering, by means of which the coal has to be reached should it exist in the position supposed. There are, however, other matters alluded to in treating the subject with which we as chemists are competent to deal. The increasing temperature of the earth as we descend from the surface has hitherto been considered as constituting a limit to the depth from which coal could be wrought. This obstacle to mining enterprise is attempted to be set aside by the allegation that the increasing temperature is due, not to internal heat of the earth, but to the weight of the superincumbent strata. In other words, the greater the thickness of rock above the coal the higher the temperature resulting from increased pressure. This, however, is equivalent to saying that the deeper you go the hotter it gets, and whether this is due to heat coming up from beneath or is caused by weight of rock pressing from above is indifferent so far as the pitmen engaged in working the coal are concerned. The proof which has to change our views on this interesting question is founded on the fact that a mine driven in at the side of a mountain at sea level has its workings showing the same temperature as Monkwearmouth

Pit, which reaches a depth as far below this level as the other mine has rock above the coal. In both cases the weight of strata above the seam is equal, and the stated temperature is to be the same, although the mountain mine is some 270 fathoms further from the earth's centre, and by so much further removed from the source of the supposed internal heat. Now, when it is remembered that 270 fathoms is but 1-13,000th part of the earth's semi-diameter, it is obvious to anyone who has studied the laws of heat that such a distance nearer or more remote can make no sensible difference in temperature, or even accepting Humboldt's estimate that our granite rocks are in a state of fusion at a depth of thirty miles, and regarding this as the source of heat, it is equally clear that under no circumstances could a difference of distance like 270 fathoms be perceived. The question, however, assumes an entirely altered aspect when you come to deal with what, for convenience, I will designate as the source of cold—which in the case under consideration is the surface of the earth—and I need not use many words to prove to a chemist that an isothermal line will necessarily vary with the configuration of the surface, subject, of course, to minor disturbances. Thus, to suppose that at 270 fathoms under the base of Mont Blanc, starting from sea level, we would meet with the same temperature as at 270 fathoms commencing on the low coasts of Holland, would be palpably incorrect—as indeed appears to be admitted by the authority we are referring to; it is, therefore, the distance from the surface, and not from the centre of the earth, that regulates the temperature of our mines. But mere pressure alone is, we know, incapable of producing heat. True it is that when matter is being compressed, the temperature is considerably raised, because, during the act you have heat in its other form, viz., motion; let motion cease, but continue the pressure, and the temperature will fall rapidly to the point it would indicate if no pressure was being exerted. The existence of heat being, however, admitted, it is sought to avoid the inconvenience its presence offers by the use of compressed air, to be used in the first instance for mechanical work underground, and subsequently its escape made available for cooling the works. In a submarine working of any extent with few shafts, an amount of ventilation requiring 200,000 cubic feet per minute may be supposed

as not an extravagant basis of estimate. Now, imagine we had penetrated to a depth where the temperature of the workings reached 100° F. To cool this quantity of air to 80°, in which men can work, represents by calculation 1,600 H.P. Sir H. Davy describes the formation of ice in a mine at Schemnitz, where air was escaping at a pressure of 126 lbs. on the inch. Our experience in obtaining air at such a density is not such as to enable us to speak with any degree of exactitude of the real power required for its generation and loss; but judging from the consumption of fuel necessary to compress the air used in the Bessemer steel process at only 20 lbs., it seems very probable that a very considerable proportion of the pit workings would be needed in order to maintain its galleries at a working temperature by any such means as those contemplated. With this I must close an address already extended, I fear, beyond the time to which I ought in strictness to have trespassed upon your indulgence. I must now leave the future of our newly-formed society in the hands of its members, upon whose assiduity in acquiring and imparting knowledge its success must entirely depend. I would remind them that in philosophical research in other departments they are surrounded by men of no ordinary stamp, who, although in most instances like ourselves, have the cares of business demanding their attention, but have nevertheless made honourably known the name of Newcastle in the annals of science. Let it be our aim to take a worthy position by their side, and let us hope that with them we may be able to achieve the beautiful with the good, according to the prayer of the Spartans of old.

Mr. GLOVER then proposed a vote of thanks to Mr. Bell for his able and exhaustive lecture. In past times they had a Hutton and a Bewick; and, in later times, a Hugh Lee Pattinson, famous for his discoveries in the chemistry of manufactures as applied to lead and the silvering process. They now had Mr. Bell, and as long as they had a gentleman among them capable of delivering an address such as they had heard that evening, they might rest assured that Newcastle would still retain her place in the world of science. (Cheers.) He asked them to support his proposition by acclamation. (The invitation was responded to with great enthusiasm.)

The motion was carried by acclamation.

The PRESIDENT responded, and said he could only once more express his feelings of gratitude for the compliment they had paid him and the kind manner in which his friend Mr. Glover had associated his unworthy name with the names of those scientific worthies who had preceded him in the good old town of Newcastle. He could assure them that any service of his to the new Chemical Society should be most cheerfully rendered. (Cheers.)

Newcastle-upon-Tyne Chemical Society.

SECOND GENERAL MEETING, JANUARY 28TH, 1869.

MR. J. GLOVER IN THE CHAIR.

The names of eight candidates for membership were read for the first time, after which the following paper was read.

ON ENGLISH AND FOREIGN ALKALIMETRICAL AND CHLORIMETRICAL DEGREES.

BY MR. JOHN PATTINSON.

THE relations between English and Foreign alkalimetrical and chlorimetrical degrees are but little known amongst merchants, chemical brokers, and others, who are in the habit of exporting large quantities of alkali, soda-ash, and bleaching powder; and it is a remarkable fact, that, even in many of the laboratories of our chemical works, these relationships are very imperfectly understood. It is a common mistake to suppose that the alkalimetrical degrees of Decroizilles represent percentages of carbonate of soda; then, again, both English and foreign chemists, who are unacquainted with the usages of the soda trade, erroneously take for granted that the English soda test is based upon the most recently established atomic weight of soda—31.

Doubtless, the want of knowledge on this subject is owing to the almost entire absence from our chemical handbooks of all information on the matter. I am not aware of any English book which gives the

value of Decroizilles' degrees; and in very few English books treating on alkalimetry is there any mention of the use of the atomic number, 32, as a basis for the test for soda in the English soda trade. I have also found a similar want of information on the relation between French and English chlorimetrical degrees, very few of our handbooks mentioning the French method of stating the value of a sample of bleaching powder.

As may be easily conceived, the want of information on these matters, in trades of such magnitude as our soda and bleaching powder trades, is a fruitful source of misunderstanding and dispute. It appears to me that our young Society, established in the midst of one of the great seats of these trades, is a fitting medium through which to diffuse the requisite information. With a view to further this object, I have made the necessary calculations, and arranged two comparative tables—one showing the relations between the amounts of soda, carbonate of soda, English degrees, and Decroizilles' degrees; and the other, the relation between French and English chlorimetrical degrees. I am indebted to M. Kuhlmann, of Lille (himself a large soda manufacturer), for the correct statement of the value of Decroizilles' degrees. (See table.)

It is, doubtless, highly desirable, for the sake of scientific accuracy and uniformity, that the atomic weight of 31 for soda, instead of 32, should be adopted in England as a basis for the test for soda. It is, however, always difficult to alter trade usages, and there are good reasons why this one should not be hastily changed. It gives to the manufacturer an advantage of 0·65 of a per cent. on a 50 per cent. alkali—that is, an alkali testing 50 per cent. by the English test, only contains 49·35 per cent. of actual soda. In the London market, it is also an established custom in the alkali trade to pay for parcels of goods only on the whole degrees, and not on the fractions, so that an alkali which contains 50·9 per cent of soda, by the English test, is invoiced and paid for as only containing 50 per cent. It will thus be seen that the disadvantage to the manufacturer of the latter custom just about counterbalances the advantage gained by the former. It would, obviously, be unfair to alter one custom, and leave the other in force. Another trade custom, affecting the chemical manufacturer to his disadvantage, is that of assuming the amount of

peroxide of manganese contained in a sample of manganese ore to be equal to the two atoms of carbonic acid evolved when the ore is tested by the ordinary oxalic acid process. These peculiar trade customs in analysis are a very frequent cause of misunderstanding and annoyance, and it would be well if they could be abolished in favour of methods which all chemists admit to be correct, according to the present state of chemical knowledge. I am of opinion, however, that these changes can only be effectually made by the chemical trade itself, and I recommend the subject to the consideration of the Alkali Makers' Association. But so long as these trade customs continue in use, it is desirable that exact information as to their nature should be attainable by all persons interested in the trade. The table here given will help to do this, so far as relates to the English soda test.

In making the calculations for the chlorimetrical table, I have adopted the combining number of 35·46 for chlorine, as given by Stas and Marignac, and have taken 0·08961 gramme as the weight of a litre of hydrogen at zero centigrade and 760 m.m. pressure. The weight of 1 litre of chlorine, at the same temperature and pressure, is, therefore, 3·17763 grammes.

COMPARATIVE TABLES OF ENGLISH AND FOREIGN ALKALIMETRICAL AND CHLORIMETRICAL DEGREES.

Alkalimetrical Table.

Percentage of Soda.	Carbonate of Soda.	English Degrees.	Decroizilles' Degrees.
30·0	51·29	30·39	47·42
30·5	52·14	30·90	48·21
31·0	53·00	31·41	49·00
31·5	53·85	31·91	49·79
32·0	54·71	32·42	50·58
32·5	55·56	32·92	51·37
33·0	56·42	33·43	52·16
33·5	57·27	33·94	52·95
34·0	58·13	34·44	53·74
34·5	58·98	34·95	54·53
35·0	59·84	35·46	55·32
35·5	60·69	35·96	56·11
36·0	61·55	36·47	56·90
36·5	62·40	36·98	57·69
37·0	63·26	37·48	58·48
37·5	64·11	37·99	59·27
38·0	64·97	38·50	60·06
38·5	65·82	39·00	60·85
39·0	66·68	39·51	61·64
39·5	67·53	40·02	62·43
40·0	68·39	40·52	63·22
40·5	69·24	41·03	64·01
41·0	70·10	41·54	64·81
41·5	70·95	42·04	65·60
42·0	71·81	42·55	66·39
42·5	72·66	43·06	67·18
43·0	73·52	43·57	67·97
43·5	74·37	44·07	68·76
44·0	75·23	44·58	69·55
44·5	76·08	45·08	70·34
45·0	76·95	45·59	71·13
45·5	77·80	46·10	71·92
46·0	78·66	46·60	72·71
46·5	79·51	47·11	73·50
47·0	80·37	47·62	74·29
47·5	81·22	48·12	75·08
48·0	82·07	48·63	75·87
48·5	82·93	49·14	76·66
49·0	83·78	49·64	77·45
49·5	84·64	50·15	78·24
50·0	85·48	50·66	79·03
50·5	86·34	51·16	79·82
51·0	87·19	51·67	80·61
51·5	88·05	52·18	81·40
52·0	88·90	52·68	82·19
52·5	89·76	53·19	82·98

ALKALIMETRICAL TABLE CONTINUED.

Percentage of Soda.	Carbonate of Soda.	English Degrees.	Decroizilles' Degrees.
53·0	90·61	53·70	83·77
53·5	91·47	54·20	84·56
54·0	92·32	54·71	85·35
54·5	93·18	55·22	86·14
55·0	94·03	55·72	86·93
55·5	94·89	56·23	87·72
56·0	95·74	56·74	88·52
56·5	96·60	57·24	89·31
57·0	97·45	57·75	90·10
57·5	98·31	58·26	90·89
58·0	99·16	58·76	91·68
58·5	100·02	59·27	92·47
59·0	100·87	59·77	93·26
59·5	101·73	60·28	94·05
60·0	102·58	60·79	94·84
60·5	103·44	61·30	95·63
61·0	104·30	61·80	96·42
61·5	105·15	62·31	97·21
62·0	106·01	62·82	98·00
62·5	106·86	63·32	98·79
63·0	107·72	63·83	99·58
63·5	108·57	64·33	100·37
64·0	109·43	64·84	101·16
64·5	110·28	65·35	101·95
65·0	111·14	65·85	102·74
65·5	111·99	66·36	103·53
66·0	112·85	66·87	104·32
66·5	113·70	67·37	105·11
67·0	114 56	67·88	105·90
67·5	115·41	68·39	106·69
68·0	116·27	68·89	107·48
68·5	117·12	69·40	108·27
69·0	117·98	69·91	109·06
69·5	118·83	70·41	109·85
70·0	119·69	70·92	110·64
70·5	120·53	71·43	111·43
71·0	121·39	71·93	112·23
71·5	122·24	72·44	113·02
72·0	123·10	72·95	113·81
72·5	123·95	73·45	114·60
73·0	124·81	73·96	115·39
73·5	125·66	74·47	116·18
74·0	126·52	74·97	116·97
74·5	127·37	75·48	117·76
75·0	128·23	75·99	118·55
75·5	129·08	76·49	119·34
76·0	129·94	77·00	120·13
76·5	130·79	77·51	120·92
77·0	131·65	78·01	121·71
77·5	132·50	78·52	122·50

The first column contains percentages of soda, calculated on the

correct atomic weight—31. These also represent what are known in France as Gay-Lussac's degrees.

The second column contains the amounts of carbonate of soda corresponding to the soda in the first column. Soda ash is sold in Germany, Russia, &c., by the percentage of carbonate of soda it contains.

The third column gives the corresponding percentages of soda according to the English test, which is based on the old atomic weight of soda—32—still retained as a trade custom in England.

The fourth column shows the corresponding degrees of Decroizilles' alkalimeter. These degrees represent the number of parts of monohydrated sulphuric acid (oil of vitriol) which can be neutralised by 100 parts of the sample under examination. Decroizilles' degrees are used in France and some other parts of the Continent.

NOTE.—In the commercial testing of samples of soda, ash, and alkali, all the soda neutralised by test acid is taken into account, and reckoned as soda in the mode of stating the results shown in the first and third columns, as carbonate of soda as shown in the second column, and as degrees as shown in the fourth column.

CHLORIMETRICAL TABLE.

French Degrees.	English Degrees.	French Degrees.	English Degrees.	French Degrees.	English Degrees.
63	20·02	85	27·01	107	34·00
64	20·34	86	27·33	108	34·32
65	20·65	87	27·65	109	34·64
66	20·97	88	27·96	110	34·95
67	21·29	89	28·28	111	35·27
68	21·61	90	28·60	112	35·59
69	21·93	91	28·92	113	35·91
70	22·24	92	29·23	114	36·22
71	22·56	93	29·55	115	36·54
72	22·88	94	29·87	116	36·86
73	23·20	95	30·19	117	37·18
74	23·51	96	30·51	118	37·50
75	23·83	97	30·82	119	37·81
76	24·15	98	31·14	120	38·13
77	24·47	99	31·46	121	38·45
78	24·79	100	31·78	122	38·77
79	25·10	101	32·09	123	39·08
80	25·42	102	32·41	124	39·40
81	25·74	103	32·73	125	39·72
82	26·06	104	33·05	126	40·04
83	26·37	105	33·36	127	40·36
84	26·69	106	33·68	128	40·67

The French degrees represent the number of litres of chlorine gas, at 0° C. temperature (32° Fah.) and 760 m.m. barometric pressure (29·92 inches), which can be obtained from 1 kilogramme of the sample of chloride of lime (bleaching powder) under examination.

The English degrees represent the actual percentage of available chlorine contained in the sample under examination. This mode of stating the results is used in Germany, Russia, America, &c.

Some discussion ensued, in which the general feeling seemed to be in favour of the employment of the true atomic weight for sodium.

Mr. R. C. CLAPHAM then read

AN ACCOUNT OF THE COMMENCEMENT OF THE SODA MANUFACTURE ON THE TYNE.

ACCORDING to the most recent printed returns* (for 1867), the raw materials now used annually on the Tyne, in producing Soda and its allied manufactures, amount to 1,070,000 tons, and consist chiefly of pyrites, salt, chalk, timber, coal, and manganese, and are of the aggregate value of £830,925. In return for this outlay there is produced:—

Soda Crystals	86,000	Tons.
Alkali	74,000	„
Bi-Carb. Soda	11,000	„
Sulphate of Copper	200	„
Sulphate of Soda (for sale)	2,400	„
Bleaching Powder	27,000	„
Caustic Soda	3,720	„
Epsom Salts	590	„
Glauber's Salts	20	„
Oil of Vitriol (for sale)	9,000	„
Hypo-sulphite of Soda	400	„
Muriatic Acid (for sale)	700	„
Chloride of Manganese	1,300	„
	216,330	Tons.

of the aggregate value of £1,929,825.

* Full particulars of the statistics of the soda manufacture were kindly laid before me by Mr. Richard Cail, Hon. Secretary of the Tyne Branch of the Soda Association; and by Mr. E. K. Muspratt, Hon. Secretary of the Lancashire Branch of the Soda Association.

The number of workmen employed in the soda works on the Tyne is estimated to be 8,000; and the number of vessels which discharged cargoes of raw materials at soda works, in 1867, was 5,000. The floating and fixed capital employed in soda works on the Tyne is from £2,000,000 to £2,500,000.

In Lancashire, the production of chemicals is nearly the same as on the Tyne; the raw materials amounting to about £869,600, and the manufactured articles to £1,883,779; thus giving the totals for the whole of England as under:—

Raw Materials	£1,700,525
Manufactured do.	3,813,604
Capital	5,000,000

Such are the main facts as they now present themselves to us. It may be interesting to inquire into the history of so important a branch of English industry, especially in this locality, for, as far as can be ascertained, the first soda works in England were commenced on the River Tyne. Before taking into consideration the artificial production of soda, we may glance for a moment at the time when alkalies were derived solely from the vegetable kingdom, and when bleaching was effected without the use of chlorine.

The GLASS TRADE was established at an early date in this district. Mr. R. W. Swinburne informs me that the first window-glass works in England were erected at the Ouseburn, by Admiral Sir Robert Mansel, in 1630; and that Mr. John Cookson put up glass works at South Shields shortly afterwards.

The SOAP TRADE was commenced by Lamb and Waldie, at Westgate, in 1770, and both branches of industry were supplied with alkali from Spanish Barilla, Russian Potashes, and Scotch Kelp—but chiefly from the latter.

The SCOTCH KELP varied in strength from 1½ to 6 per cent., but did not average more than 3 per cent. of alkali; and the Barilla contained about 25 per cent. of alkali. The neutral salts were Sulphates and Chlorides of Potassium and Sodium.

From the first production of Kelp in Scotland, in 1730, to the end of the last century, its price gradually advanced from £2 5s. to £11 per ton; and the price of Barilla at the same date was about £45 per ton. These prices are equal to £180 or £200 for a ton of Soda Ash, which is at present worth £8.

The only method employed, as late as a hundred years ago, in bleaching the British-made linens, and calicoes, &c., after boiling them in leys from Kelp, was to saturate them with sour milk, and expose them for long periods to the action of the air; but, on account of the uncertainty of the climate, it was necessary that the best cloths should be sent to Holland, and, after a summer's absence, they were returned for use in England.

Writing paper was then only made from the whitest rags; and the expense of bleaching was found to be as great as that of obtaining alkalies. It was under these circumstances, and when the supply of high-priced and crude materials was found utterly inadequate to meet the increased demand for chemicals, that a *new* manufacture was commenced in this district.

It happened that a north-countryman (the late Mr. William Losh) was living in Paris at the time of the Revolution, and his attention, as a chemist, was particularly directed to the artificial production of soda. France, being then at war with almost every other Continental nation, all her external supplies of potash, soda, and nitre were cut off. Under these circumstances, in 1792, the Committee of Public Safety instituted an inquiry into the best method of manufacturing soda from common salt. Up to that time various methods had been proposed, both in England and France.

As far back as 1777, Malherbe had suggested to the French Government the decomposition of sulphate of soda, by means of carbon, tin-parings, and iron; and, in 1781, Alexander Fordyce patented a plan in England for using oxide of iron.

The employment of the metals, or metallic oxides, was discovered in practice to present many difficulties, and the suggestions were not carried out. It was Le Blanc, Dizée, and Shée, joint associates, who suggested to the French Government the use of lime, as a means of separating the sulphur in decomposing sulphate of soda; and, it is scarcely necessary to add, that their method has been adopted in practice, both on the Continent and in England.

With a zeal characteristic of the times, a small manufactory was erected at the national expense, at Franciade, near Paris, and in the year 2, or 1794, it was fully at work. The operations did not, however, proceed satisfactorily. The quantity of soda turned out was smaller than expected, and many difficulties presented themselves.

Le Blanc and his fellow associates were discarded by their Government, and became so embarrassed and discouraged, that they appealed for aid to the English Government, and received some pecuniary assistance. Le Blanc died in poverty in a French hospital; but it is satisfactory to know, that statues were erected to his memory at Rome and Greece.

It was not till twenty-eight years after the time I have named that Le Blanc's process was adopted in England.

About 1793 or 1794, Mr. Losh and the Earl of Dundonald commenced a series of experiments in the manufacture of soda, at Bell's Close, near Scotswood, which resulted in the Earl taking out a patent, in 1795, for "Treating Neutral Salts to obtain Alkalies." Soda was made at Bell's Close during these years, but to what extent, or by what process, there is no record.

The works conducted at Bell's Close were removed in 1797 to Walker; a lease having been signed on the 29th September for twenty-one years, the object being to obtain a more eligible site, and to make use of a salt spring recently discovered there. A coal pit, called the King Pit, had been sunk in 1788 by the late Mr. Barnes, and an engine erected by the well-known engineers, Bolton and Watt, in the same year to work the coal. This engine was put up on the old principle of the "sun and planet motion," and is still at work.

In the shaft of this pit, the spring was discovered, and on the 24th June, 1798, a lease was signed with the Corporation of Newcastle, for its exclusive use, and an agreement entered into with the owners of Walker Colliery to pump the brine. A 4-inch lead pipe was put down on the outside of the metal tubbing of the pit to the salt spring, and the engine did the double work of drawing the coal and pumping the brine.

The salt spring was analyzed by a Mr. G. Woods, and gave as under:

CONTENTS IN 1,000 GRAINS OF BRINE.

Chloride of Sodium	32	Grains.
Chloride of Calcium	10	„
Chloride of Magnesium		
Carbonate of Lime	1	„
Carbonate of Iron		
Silica		
	43	Grains.

Its strength was 7½° upon Twaddell's Hydrometer, and it was conveyed in wood spouts from the pumps to the evaporating pan, near the pit shaft.

The price of small coals used in evaporation, was 5s. 6d. per chaldron, and the total cost of making the salt, was as under :—

	s.	d.	
Coals and Cartage	21	9	per Ton.
Wages	6	1	„
Pumping	6	0	„
Total	33	10	per Ton.

The excise duty upon salt, at the date I am now speaking of, and for many years afterwards, was from £10 to £36 per ton, according to the exigencies of the wars in which England was then engaged, and so scarce and dear was salt to the working classes and the general public that it was a common practice to bore holes in the wooden spouts, and convey the brine secretly to the workmen's houses to evaporate for domestic use. These high duties were not finally repealed till 1823.

Through the influence of the Earl of Dundonald with the Government, an order in Council was given that the salt made at Walker might be used in the manufacture of soda, free of duty, with a stipulation that soot or ground coal must be put into the pans, to prevent its use for other purposes; and an exciseman lived on the premises to watch proceedings. This peculiar privilege was found to be a great boon in establishing the soda trade at Walker.

The quantity of salt produced at first was not more than two or three tons weekly, but was afterwards increased to eight tons; and, as far as can be ascertained, this was the most that was made, which shows to what a limited extent chemical operations were then conducted. These salt pans continued at work till 1826. The first importations of Cheshire salt, at 12s. and 13s. per ton, commenced in 1825, and soon superseded the salt made at Walker.

Various manufactures appear to have been carried on at these works :—From 1800 to 1822 sulphur works were in operation, making about twenty tons yearly of roll and flour sulphur. Wood charcoal and pyroligneous acid were also made; and from the commencement of the works till 1806 the smelting of lead was conducted; at the

same time the separation of the silver, and the production of litharge. In connection with the latter, the manufacture of soda commenced.

Other processes in making soda were also conducted, which will be described, but all of which have been long since abandoned; but they may be interesting to refer to, as forming part of the history of the introduction of the soda trade into the district.

(*a.*) Mr. Losh stated to the writer that one of the first processes adopted was the decomposition of salt with litharge, and in most of Lord Dundonald's papers, which are referred to also by the *Annales de Chimie*, and other French periodicals, he recommends 75 parts of oxide and 25 parts of salt; but it is doubtful whether these proportions were used at Walker. Large quantities of pig lead and lead ore were purchased to produce litharge; but the only large separate experiment I can find, in the late Mr. Thomas Bell's handwriting, gives 10 per cent. of salt, which produced 15 tons of British ashes, and left a profit of £20 per ton.

The sub-chloride of lead resulting from this decomposition, when calcined, produced a deep yellow pigment; but from what can now be ascertained, its manufacture does not appear to have been prosecuted, the lead being simply reduced, and sold, as soft lead, to Walker, Ward, Parker, and Co., and others, at about £30 per ton; and the silver to a Mrs. Robinson, at 6s. 6d. per ounce.

The soda obtained in solution from this process was evaporated to dryness, mixed with sawdust and calcined, with the object of converting it into a carbonate, from which crystals of soda were made as far back as 1799, if not in 1798.

(*b.*) The chief operations, however, appear to have been in carrying out Dundonald's patent, already referred to—the double decomposition of the salts of potassium and sodium. From 1802 till 1815, Russian potashes were purchased to a considerable extent, and then cost £40 per ton. The mixture generally consisted of equal weights of salt and potash, which were first dried in a furnace, they were then mixed with slaked lime and coal, and balled. The solution from the ball was evaporated till the chloride of potassium could be separated by two crystallizations. The remaining solution was then dried down and furnaced, and was again dissolved to make crystals of soda. 40 cwts. of salt and potash produced 20 cwts. of muriate of potash, and 20

cwts. of British ashes; or, if crystallised, 30 or 40 cwts. of soda. The material called British ashes was sold as far back as 1804, and for many years afterwards to Clapham, Foster, and Co., at the Ouseburn, from £16 to £20 per ton. The crystallizing was conducted at that time in small flat lead vessels, and only during the winter months; and the soda made by this process was sold as "potash soda."

(*c.*) A process which appears to have occupied a great deal of attention, and which continued from 1812 to 1822, was the employment of soaper's salts in making soda. The Tyne and Scotch soapmakers generally made their soaps with kelp, which, as before explained, contained large quantities of sulphate of soda, chloride of potassium, and common salt. The leys from the soap works were evaporated to dryness, and sent to Walker, and this rough salt was worth £7 per ton. It appears to have been always carefully analyzed according to the old methods, and contained :—

Chloride of Potassium	50 to 60 per Cent.
Sulphate of Soda and Common Salt	20 to 30 „
Water and Residue...	10 to 20 „

It was mixed with its proportions of nitre cake (sulphate of potash) and dissolved in water, and evaporated to the crystallizing point. The chloride of potassium was separated, and the solution, consisting chiefly of sulphate of soda, was evaporated to dryness, and used as hereafter described.

(*d.*) Nitre cake, obtained from the nitric acid makers of Glasgow, London, and Ireland, was also much used. It cost £14 per ton, and consisted, at that time, of bi-sulphate of potash, and was treated as under :—10 cwts. of nitre cake was ground with 1 cwt. of salt, it was then furnaced, 5 cwts. of salt was added, and the whole was mixed with 7½ cwts. of coals and 8 cwts. of slaked lime, and was balled. The ball was digested in water, and very pure chloride of potassium was separated, and the carbonate of soda was crystallized. In 1810, a parcel of 5 tons of Irish nitre cake, and 3 tons of salt, produced 42 cwts. muriates, and 64 cwts. British barilla, and left a profit of £10 per ton on the latter.

(*e.*) In making glass at Messrs. Cookson's, and other works, with kelp, sulphates and chlorides of sodium and potassium floated on the

top of the pot, and were skimmed off and sold under the name of "sandiver." It consisted of about

50 per cent. of Sulphate of Soda,
40 „ „ Sulphate and Muriate of Potash,
10 „ „ Sand and Glass,

and was bought by Mr. Losh, for which he paid £8 per ton; and it was used in the works at Walker, in the same way as the nitre cake.

These different operations (marked in the margin *a*, *b*, *c*, *d*, and *e*) were carried on according to the price of the materials at the time, and the chloride of potassium produced, was sold to Mr. Mackintosh, of the Hurlet Works, near Glasgow, or to the Marquis of Normanby, at Whitby, at prices varying from £12 to £20 per ton. Both the Marquis and Mr. Mackintosh assisted in supplying a waste product from their works, called "alum slam," a kind of basic sulphate of alumina which was employed at Walker in decomposing salt.

The sulphate of soda produced from any of the above methods was decomposed by balling and converted into carbonate. This operation was then conducted in the following manner:—The sulphate of soda, slaked lime, and coal, were each separately ground and finely sifted, and about half-a-hundredweight of sulphate of soda, with its proportion of coals and lime, was used to make a ball. The charge was mixed with water, or weak soda liquors, and made into the form of a paste, and was then placed upon what was called the shelf of the furnace, and, when ready, was pushed forward into the working bed. Six of these balls were worked in a shift of twelve hours; but, in time, ten were accomplished. The weight was increased to one hundredweight of sulphate in 1820, and these small weights continued to be the practice till 1838, after which time they were gradually increased to three hundredweights.

These were the methods adopted in making soda on the Tyne till 1821, and the new plan, subsequently practised, will be referred to hereafter. From the commencement of the works till 1820, a part of the produce was sold as British ashes, to the extent of about forty tons annually, testing from 15 to 20 per cent. of soda, and bringing £16 per ton. When the new system of working commenced, ball

soda was sold to a large extent till 1832, at £14 per ton, when both products were superseded by alkali and crystals of soda. It is rather remarkable, also, that in spite of the great increase in the soda trade of late years, I find from Mr. R. Hunt, of Jermyn Street, that Spanish barilla was imported, in 1864, to the extent of 1,262 tons, and Scotch kelp was brought to the Tyne in 1842, and refined.

As sulphuric acid occupies so important a place in the present system of making soda, I have taken great pains to obtain the best information concerning its introduction into England; and, I believe it may be safely stated, that the whole of what was made up to 1730, was produced from sulphate of iron. Copperas works had existed in England from an early date; in 1748 they began at Hartley, and in 1797 at Walker. The copperas, after being dried, was placed in brick ovens, to which a strong heat was applied, the acid distilled off, and was condensed, and formed an impure oil of vitriol. If one ton of copperas produced 1½ cwt. of acid, it was considered a fair result. The price was 2s. per lb., or £220 per ton, the present price being only £6 per ton.

The next step in advance was the attempt to make oil of vitriol from sulphur. This was done in 1736. The plan was as follows:— The sulphur and nitrate of potash were suspended on an iron tray, in large wide-mouthed glass globes, partly filled with water, and was set fire to by a red hot iron, and the mouth was then closed. As soon as the charge was completed, another took place, till the acid was found to be sufficiently strong, when it was removed into a glass retort and concentrated much in the same manner as at present. This acid also brought 2s. per lb., and was in much demand, on account of its purity, for several years.

In 1746 a Dr. Roebuck, of Birmingham, stated to be a man of considerable scientific attainments, put up the first leaden chambers in England, as a substitute for glass globes. These were six feet square, and the charges of sulphur and nitre were burnt in the chambers, in the same manner as in the glass globes. The success of these chambers was so great that others were erected at Preston Pans, in Scotland, in 1749; and shortly afterwards at Bridgenorth and at Bewdley, and in London, in 1772, and in Lancashire, in 1783. The manufacture of oil of vitriol, by the new method, gradually superseded

the old one from copperas; and the total quantity from both sources had so far increased that, in 1790, England became for the first time an exporter, to the extent of 2,000 tons, instead of a large importer of oil of vitriol.

The first sulphuric acid chambers were put up on the Tyne, at Bill Quay, about 1812 to 1815, by Doubleday and Easterby. The acid produced was sold at first at 3d. per lb., and afterwards at 2d. per lb.; a part of it was also used to decompose soapers' salts, which were taken down the river to Bill Quay from this firm's soap works, in the Close, to avoid any annoyance to the Corporation authorities, in the Mansion House there. The sulphate of soda produced was converted into British barilla, and used in the soap works.

At the time the Bill Quay works began to make acid the plan still adopted was to burn the sulphur and nitrate of potash together. But improvements had so far advanced that this operation was conducted in a separate furnace, and the gases evolved were conveyed by a pipe into the chamber. No steam was then used.

When the charge was burnt off three hours were allowed for the condensation of the gases, and the doors of the chambers were opened to what was called sweeten the chamber, and then the operations were continued as before. The acid was then generally brought up to 70° or 80°, when it was run out, and a fresh supply of water took its place. The produce of acid by this plan was not more than 36 or 40 cwts. from a ton of sulphur, or 180°/₀ to 200°/₀.

In 1821, Mr. Losh erected acid chambers at Walker for the purpose of decomposing salt, and manufacturing soda on the French plan; and Mr. Charles Tennant commenced similar operations in Glasgow, in 1819, so that these appear to be the first two alkali works commenced in this country on the French plan. The chambers erected at Walker were about 25 feet long by ten feet wide and ten feet high; and two were put up at first, in the lower part of the works, by a plumber from Glasgow. In the following year two larger chambers were erected, and the four chambers were all that were put up for some years. The price of sulphur at that time was £7 per ton (the duty being £15 per ton, which was remitted), and nitrate of potash was £32 per ton.

Improvements had so far advanced that the nitrate of potash was

decomposed by sulphuric acid in a separate pot, steam was introduced into the chambers, and at the same time continuous working of the process, which very largely increased the produce of acid.

During the change of manufacture, large quantities of oil of vitriol were purchased, which cost £18 10s. per ton. Owing partly to the high price of nitre, and the large quantity then used, the cost of making sulphuric acid could not be less, for the first few years, than £6 or £7 per ton.

The sulphur used was all derived from Sicily. The duty of £15 per ton was reduced in 1825 to 10s. per ton. The price delivered in the Tyne, after the reduction of the duty, was about £6 to £8 per ton. This state of things continued till 1838, when the King of Sicily entered into an agreement with Messrs. Taix and Co., of Marseilles, for the exclusive sale of all the sulphur produced in his dominions. The effect of this was an immediate advance to £13 or £14 per ton, and a great depression and stagnation in the alkali trade. Under these circumstances, the attention of chemists was directed to pyrites, as a substitute for sulphur, which was found in abundance in Cornwall and Ireland. This mineral was first imported into the Tyne in 1840; and, in June of that year, was used by Mr. John Allen, at Felling Shore, and in September, by Messrs. H. L. Pattinson and Co., at Felling.

Foreign pyrites, of higher strength than the English or Irish, were imported in time :—*In 1856, Spanish pyrites were first used; in 1858, Belgian pyrites; in 1861, Westphalian pyrites; and the same year Norwegian.

Perhaps nothing can give a more correct impression of the progress of chemical manufactures than the production of sulphuric acid. The Custom House gives very imperfect returns of imports earlier than 1840; but from private sources it may be safe to say, that in 1820 not more than 200 or 300 tons of sulphuric acid were produced on the Tyne. Parks of London, himself an acid maker, and a close observer, states that in 1815 not more than 5,000 tons of acid were made in the whole of England.

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* I am indebted for this information to Mr. A. S. Stevenson, Mr. H. Scholefield, and Mr. H. Clapham, who are all extensive importers of foreign pyrites.

In 1830, the produce of acid on the Tyne may be calculated at about 3,300 tons, and advancing to 1867; and acting on more certain returns, it appears that the production of sulphuric acid has increased to 140,000 tons, which is certainly an enormous increase.

The production of crystals of soda being confined to the Walker works till 1827, when Mr. Cookson came into the market, I am able to give the quantities made on the Tyne in the following years. The crystallizing was chiefly confined to the winter months, viz., from October to May:—

1799—	10	tons	crystals of soda were made.
1800—	23	„	„
1805—	57	„	„
1810—	66	„	„
1815—	130	„	„
1820—	121	„	„
1825—	406	„	„

In 1830 the production at the Walker works had increased to 986 tons, and, in addition to this, three other makers had produced about 2,300 tons, making the total production, in 1830, about 3,300 tons. The returns for some years after cannot with accuracy be made out; but in 1867, as before stated, the produce of soda crystals on the Tyne was 86,000 tons, besides a very large quantity of alkali, showing an enormous increase in seventy years.

It may also be interesting to refer to the prices of soda for the last seventy years;* and, in comparing the prices, the small quantities produced, and the general circumstances of the times, should be taken into consideration:—

In	1799—	£50	0	0	per ton.	
„	1800—	44	10	0	„	
„	1805—	47	1	0	„	
„	1809—	62	17	0	„	(*highest price to* £71.)
„	1810—	59	1	0	„	
„	1815—	54	10	0	„	
„	1818—	40	5	0	„	
„	1820—	36	10	0	„	
„	1825—	29	0	0	„	
„	1830—	18	5	0	„	

* I am indebted to Mr. Thomas Bell for much of this information.

In	1835—	11	0	0	per ton.
„	1840—	10	2	6	„
„	1845—	6	10	0	„
„	1850—	5	10	0	„
„	1855—	5	2	6	„
„	1860—	6	15	0	„
„	1865—	4	15	0	„
„	1868—	4	2	6	„

From 1779 to 1825 the casks were charged extra, and averaged from 25s. to 30s. per ton.

In looking back, for about the same, I find that the wages of ball-furnacemen, and other skilled soda men, were as follows:—

In	1810	the wages were	14s.	per week.
„	1817	„	16	„
„	1824	„	16	„
„	1830	„	17	„
„	1835	„	17	„
„	1840	„	21	„
„	1843	„	21	„
„	1850	„	22	„
„	1860	„	24	„
„	1868	„	27	„

At the same time that wages in soda works have nearly doubled, during this time, the quantity of work turned out by a workman has more than kept pace with the wages paid, and may be said to be at least four times as great. This appears to be due jointly to the increased skill of the workman, and to the better appliances and apparatus provided by the manufacturer.

From the commencement of Le Blanc's process, even to the present day, the difficulty has been to devise furnaces or apparatus to meet the scientific improvements suggested.

In decomposing salt, the first furnaces were constructed of lead, lined with brick work. The charge of salt was 2 cwts., and the sulphuric acid required was slowly poured upon it from a carboy, through a hole in the top of the furnace. The hydrochloric acid produced was allowed to escape into the air by means of a short chimney. The operation was completed in about three hours, when a plug was withdrawn from the side of the lead pan, and the semi-fluid mass allowed to run into a lead cooling vessel, where it formed itself into a hard cake, resembling ice or glass. This was broken

up and furnaced for sulphate of soda; but as these lead furnaces were very soon worn out, a fire brick furnace was substituted in 1828, long known as a "dandy furnace." A great improvement upon this plan was introduced in 1840 by Mr. John Lee, who suggested the use of a metal pan, about nine feet diameter, in the place of the furnace; and its adoption has led to a large saving in the sulphuric acid formerly employed.

On the repeal of the duty on salt, in 1823, a great development in soda works shortly followed. Next to Mr. Losh, came Mr. T. H. Leighton, who commenced at Blyth in 1822, and used salt made from sea water, and decomposed by copperas. He afterwards purchased sulphuric acid of Doubleday and Easterby, at Bill Quay, and from Tennant and Co., at Glasgow, which cost £18 10s. per ton.

Cookson and Co. (now Jarrow Chemical Co.) began, in 1823, by the erection of chambers and furnaces in the middle of South Shields; they removed a few years afterwards to Jarrow Slake, where very extensive works are now carried on.

John Allen commenced at Felling Shore, in 1827; A. Clapham at Friar's Goose, in 1829; C. Attwood (now Allhusen and Sons) at South Shore, in 1830; and H. L. Pattinson and Co. at Felling, in 1834.

A similar development took place in Lancashire, Muspratt having commenced in 1824, Crosfield and Gamble in 1827, and many others followed.

At the same time that the increase in the manufacture of soda was going forward, little or no attention was paid to the condensation of the hydrochloric acid, and large quantities passed off into the air, doing considerable damage to vegetation, and resulting in heavy legal expenses. Various methods were attempted to condense it. The first experiments at Walker were made in 1827; and later on, about 1835, Mr. W. S. Losh put down long underground flues, into which jets of water were forced by means of an engine. A large wood chamber was also put up for the gas to pass into, which was also supplied with water by the same means. This was in imitation of the lead chambers used in France as condensers. Mr. John Allen also tried long flues at the Felling, one running nearly all round his works. Towers constructed of stone, and filled with glass or

flints, were also put up; but for the most part all these were failures, or at least were imperfect, and to get over the difficulty high chimneys were erected—one object being to spread the gas over a greater surface.

It was in these difficulties that Mr. Gossage (to whom the Alkali Trade is so much indebted for many other improvements) took out a patent in 1836, for condensers packed with coke. His plan was generally approved by manufacturers; and, under ordinary circumstances, it is capable of condensing the whole of the hydrochloric acid produced. Mr. T. H. Burnett also took an active part in the introduction of condensers in this district; but it was not till 1843 that condensation was generally carried out on the Tyne, and even some years later than that date, when the perfect system which is now adopted came into general use.

I have already referred to the time when bleaching was effected without chlorine. Berthollet was the first to suggest in a paper to the French Academy, the use of chlorine in bleaching. This was in 1785. In 1787 Professor Copeland and the Duke of Gordon commenced works in Aberdeen to make chlorine in large glass Woolfs' apparatus. This was afterwards improved by the apparatus being made of hard wood. The mixtures used were,

½ lb. of good Manganese.
1 „ of Salt.
12 oz. of Sulphuric Acid.
12 „ of Water.

Bleaching by means of chlorine was introduced to the manufacturers of Glasgow in 1789 by Mr. Watt the engineer, and it soon found its way into Lancashire. As the demand for chlorine increased, the apparatus for its production was enlarged, and a vessel constructed of strong lead, fixed in a metal jacket, was substituted, a stone agitator being used to mix the charge of salt, manganese, and sulphuric acid. The chlorine passed off into water, and was sold in that form in carboys to the bleachers. It was ascertained by Javelle, in 1790, that a little caustic potash added to the water was the means of adding strength to the bleaching liquor, and this plan was introduced into Liverpool, and a small work established for its manufacture, in 1792. A further improvement was effected by Mr. Charles Tennant,

of Glasgow, in 1798, who introduced lime into the water, in the place of potash; and, in the following year, he took out a patent for dry hydrate of lime in absorbing chlorine, which soon entirely superseded all other plans previously in operation.

Bleaching powder was first made on the Tyne by Losh, Wilson, and Bell, in 1830. The first operations were conducted by using muriatic acid, at 30° Twaddell (made in the works by retorts), and finely ground English manganese, then testing about 84 per cent. Two lead stills were put up at first, and two chambers made of wood, and covered with pitch. In time two other stills were erected, and these four stills did all the work required till 1843, when the concern was handed over to the present Alkali Company.

In 1830, the proportions for a charge were,

10½ cwts. of Lime,
12 „ Manganese,
24 „ Muriatic Acid,

and produced 13 cwts. of bleaching powder, testing about 26 per cent.

In the following year (1831), the mixture was changed to,

10 cwts. of Manganese,
10½ „ Lime,
35 „ Muriatic Acid,
7½ „ Sulphuric Acid,

and produced 15½ cwts. of bleaching powder.

In 1832, the mixture was again changed to,

9 cwts. of Manganese,
10 „ Common Salt,
9 „ Sulphuric Acid,
10½ „ Lime,

and produced 14½ cwts. of bleaching powder.

From April, 1832, the process consisted in using salt, manganese, and sulphuric acid.

On September 9th, 1843, the process was changed to the present plan by using hydrochloric acid, obtained by condensation and manganese, the quantities produced at first were as under,

In 1830—Not known.	In 1832—109 tons.
In 1831— 73 tons.	In 1841—370 tons.

In 1867 (as before stated) the produce of bleaching powder on the Tyne had increased to 27,000 tons.

When Mr. Charles Tennant commenced making dry bleaching

powder, in 1799, the price was 1s. 6d. per lb., or £168 per ton, and the following are the prices since that time *—

In 1805—	£120	0	0	per Ton.	In 1843—	£21	0	0	per Ton.
„ 1810—	84	0	0	„	„ 1846—	18	10	0	„
„ 1815—	80	0	0	„	„ 1847—	14	10	0	„
„ 1820—	47	0	0	„	„ 1848—	13	10	0	„
„ 1825—	27	0	0	„	„ 1850—	13	15	0	„
„ 1830—	23	0	0	„	„ 1855—	10	15	0	„
„ 1832—	21	0	0	„	„ 1857—†	13	10	0	„
„ 1835—	23	10	0	„	„ 1860—	11	0	0	„
„ 1840—	21	0	0	„	„ 1868—	10	12	0	„

Up to 1815, an excise duty of £30 per ton was laid upon the waste products from bleaching powder stills; and, therefore, it was not till sometime after that date that the sulphate of soda contained in the still liquors was used in making alkali. Mr. Tennant was one of the first to begin, and considerable quantities were made from this source, and many patents taken out for so doing.

Messrs. H. L. Pattinson and Co. were the first to put up stills on the present system. This was in 1842. In 1846, Mr. W. W. Pattinson took out a patent for the internal application of steam to the stills, which has added much to the efficiency of working.

I must now bring this paper to a close. I am aware of many omissions and that several interesting processes connected with the soda manufacture might have been referred to; but it has already extended beyond my original limits. Many of the facts recorded I have obtained by personal application to manufacturers, and in all cases I have received the fullest assistance.

On the motion of the CHAIRMAN, the thanks of the meeting were given to the authors of both papers.

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* I am indebted to Mr. John Tennant for this information and other particulars.

† During 1857 the price rose to £18 per ton.

Newcastle-upon-Tyne Chemical Society.

THIRD GENERAL MEETING, FEBRUARY 24TH, 1869.

MR. J. GLOVER IN THE CHAIR.

Eight new members were elected; the names of seven candidates for membership were read for the first time; after which,

Dr. SCHWARZ read a paper—"On the Toxicology of the Metallic Cyanides," which dealt chiefly with the medical aspect of the question, and detailed at some length the symptoms and post-mortem appearances consequent on small doses of gold-potassium cyanide, &c.

On the motion of the CHAIRMAN, the thanks of the meeting were given to Dr. Schwarz.

Dr. LUNGE read for Dr. CLEMM the following paper on

A NEW VOLUMETRIC METHOD OF ESTIMATING SULPHURIC ACID AND SULPHATES.

THE best among the volumetric methods for the estimation of sulphates, which are now being used along with the determination by weight, are those of R. Wildenstein and of Carl Mohr. The first method consists in precipitating the sulphuric acid by a standard solution of baric chloride, observing the point when no more cloudiness is produced by the latter, and calculating the sulphuric acid from the volume of the baric solution employed. Since it is extremely difficult to recognise the point when the precipitation has just been completed, Wildenstein has proposed to use a so-called "syphon filter," by means of which small portions of the solution under trial are filtered

after each addition of baric chloride, in order to try with the clear filtrate whether the precipitation is complete or not. This plan, however, even if pursued exactly according to the description of the author, is rather troublesome, and takes at least half an hour for each estimation.

In some alkali works this plan is followed in its principle, but somewhat modified. The liquid to be examined is saturated with hydrochloric acid and kept boiling in a porcelain capsule, and a standard solution of baric chloride is added to it gradually (say $\frac{1}{4}$ CC at a time) till no more precipitate is formed. This point is found out by dipping a very small filter in the liquid and dropping two clear drops on a glass plate resting on a dark ground. To one of these drops there is added a drop of sulphuric acid, to the other a drop of baric chloride solution. If the latter produces a cloudiness, this shows that there is still some sulphate in excess; if, however, the former does so, an excess of the standard solution must have been already employed. This method does not take much time to perform, but it can make no claim to great accuracy even in skilled hands, from various reasons.

Much more accurate is the method proposed by Carl Mohr. He decomposes the sulphates by a measured excess of a standard solution of baric chloride, takes away the excess of the latter by precipitation with sodic or ammonic carbonate, in the shape of baric carbonate, filters and washes the latter, dissolves it in standard nitric acid and measures the excess of the latter with a standard solution of potash or soda. As it is in this way found how much baric carbonate has been formed, and as the original amount of baric chloride employed is known it is easy to calculate the amount of sulphuric acid from the difference between the two quantities, the quantity of baric chloride not being converted into carbonate necessarily existing as sulphate.

Now, my plan goes a step further. I precipitate the excess of baric chloride by a known quantity of sodic carbonate, and I estimate how much of the latter remains unaltered. My process is, in short, as follows:—The solution to be examined is made blue with a few drops of litmus solution and exactly neutralised, which is, of course, very rapidly done. Then a certain quantity of a standard solution of baric chloride is added, sufficient to precipitate all the sulphuric acid; then a measured quantity of sodic carbonate solution of known

strength is added to precipitate all the baryta, and lastly the excess of the sodic carbonate is titrated with standard sulphuric acid. In all these reactions no soluble salt is formed which might influence the final litmus reaction. Sulphates, the metals of which are precipitated by sodic carbonate, are first treated by the latter; the sulphuric acid appears then in the filtrate as sodic sulphate, and this filtrate is neutralised and treated as above. It need hardly be mentioned that this process is not applicable in presence of phosphates, oxalates, or other salts which precipitate neutral baryta solutions.

I prepare the necessary standard solutions as "fifth normal" ones, that is to say: 1st, a solution of 20·8 grammes Ba Cl_2 in a litre, 2nd, 10·6 grammes Na_2 CO_3 (very conveniently just one equivalent of the baryta used) in a litre, 3rd, 8·0 grammes SO_3 in a litre of liquid. My experiments have shown me that this strength is very suitable for the accurate estimation even of small quantities. The solutions exactly neutralize each other bulk for bulk. Thus, if 20 CC baryta solution are added to a liquid free from sulphates, and 20 CC soda solution are added thereto, the filtrate, made blue by litmus, will be reddened by the first drop of sulphuric acid. If, however, part of the baryta has been taken up by sulphates, an exactly corresponding quantity of sodic carbonate will remain free, and will be indicated by the standard sulphuric acid. Suppose, for instance, that 6 out of the 20 CC Ba Cl solution had been decomposed by sulphates there will be again 6 CC sodic carbonate solution in excess afterwards, if 20 CC of this latter had been added; and lastly 6 CC of sulphuric acid will be required to restore the neutrality of the liquid. The acid used in the last stage exactly corresponds to the quantity of sulphates originally present; each CC of it, therefore, indicates 0·008 grammes of SO_3, either as free acid or as sulphate in the original liquid. A practical instance will make this still clearer. I dissolved 0·5 gr. pure sodic sulphate in a 200 CC flask; the solution, of course, required no neutralizing. To this I added 50 CC of my standard ($\frac{1}{5}$ normal) baric chloride, then 50 CC of the $\frac{1}{5}$ normal sodic carbonate, filled up to the marks, mixed the contents of the flask well by shaking, and filtered half of it into a 100 CC flask. The filtrate required to neutralize it 17·5 CC of its $\frac{1}{5}$ normal sulphuric acid. To apply this to the original bulk of 200 CC the figure has to be doubled = 35·0 CC, and it now shows 35·0 × ·008

grammes, or 0·280 gr. SO_3. The quantity of SO_3 in 0·5 pure Na_2SO_4 is 0·281.

The most important advantage of my plan over that of Mohr is, that I need not wash the precipitate of baric carbonate and sulphate, which is always a tedious operation; I avoid this by filtering off half of the liquid. Mohr has no such choice, as he does not use the liquid, but the precipitate. Besides, I have not to titrate a liquid made muddy by baric sulphate, which, when present in any quantity, always somewhat impedes the accuracy of the litmus test.

The CHAIRMAN inquired if the presence of lower sulphur salts did not interfere with the application of the process.

Dr. LUNGE explained that they were effectually disposed of by the preliminary treatment.

Mr. HILL inquired if the presence of sodium silicate did not affect the results in tank liquors, &c., and with reference to the determination of sulphur in pyrites, whether no basic sulphates accompanied the ferric hydrate.

Dr. LUNGE said, with regard to the first question, the point had not been specially attended to. He considered that the test analyses showed that any error from the second source was unappreciable.

Newcastle-upon-Tyne Chemical Society.

FOURTH GENERAL MEETING, MARCH 25TH, 1869.

ISAAC LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

Seven new members were elected; the names of two candidates for membership were read for the first time; after which, Dr. LUNGE read the following

NOTES ON THE PROGRESS OF ANALYTICAL CHEMISTRY IN FOREIGN COUNTRIES DURING 1868.

(*Extracts from Fresenius' Zeitschrift für Analytische Chemie.*)

THE following extracts only touch on those topics which I thought of most interest for the Society, and, of course, only refer to such papers as have not yet, to my knowledge, appeared in any English periodical. I must ask for indulgence if I should err in this respect and refer to papers already published in this country. Some of them I shall only mention by name, others I shall treat at a little greater length, and a few will be rendered even more completely, according to the degree of importance which the papers seem to me to possess.

Rheineck (51*) gives a process for the alkalimetric determination of phosphates. They are converted into tricalcic phosphate, observing certain precautions not to obtain calcic carbonate besides; the tricalcic

* The figures in parentheses refer to the pages in the original.

phosphate is dissolved in standard hydrochloric acid, and the excess of the latter found by adding standard soda solution till insoluble flakes appear in the liquid. Two equivalents of H Cl correspond to one of $P_2 O_5$. The method does not give very accurate results, but it will do when many rough estimations are required, and a cheaper reagent than the uranic acetate is desired.

Wihlert (58) describes an apparatus for the estimation of carbonic anhydride and sulphuretted hydrogen in coal gas. Its principle is to remove those gases by absorbents from a known volume of coal gas, and to measure the contraction. He absorbs the CO_2 by caustic potash in sticks, and the $H_2 S$ by manganic peroxide. The apparatus seems to be rather ingeniously contrived, and the author asserts that it is possible to make the estimations with it not only very quickly but also within 0·1 per cent.

Storch (60) describes an extremely simple apparatus for estimating fatty matter by extraction with ether.

Wittstein (84) describes a modification of the burette. Everybody knows how much trouble the pinch-cocks as well as the India-rubber 'oints give. Wittstein does away with both of them altogether. This burette ends in a very fine point, so that a pin only enters it about 2^{mm} deep. The burette then only empties itself drop by drop when its mouth is free, and it is completely stopped when covered by a small piece of ground glass plate, which is slightly greased with butter on its under side, and is weighted by a piece of lead. If it is desired to empty the burette more quickly, an India-rubber cap is put on and compressed with the fingers; in order to fill the burette it must be closed with the finger or by putting it into a porcelain capsule.

Cooke (86) describes a eudiometer, which is extremely simple and inexpensive, as it requires no mercury trough, and only a few cubic centimetres of mercury.

Scherbler (88) has constructed a regulator for air baths, which acts with less fluctuation than that constructed by Bunsen and Kemp. Its principle is that a platinum wire, when touched by the mercury rising in a wide thermometer tube, at any point of temperature desired, establishes a galvanic current, and causes a plate to be attracted by an electro-magnet; this plate then very nearly, but not quite, shuts

off the supply of gas to the lamp. (Messrs. Mawson and Swan keep this kind of regulators in stock.)

Stolba (102) recommends the estimation of lead in soluble salts by precipitation with zinc. The method does not seem very generally applicable.

Vogel (149) remarks that in estimating argol by calcining it, some potassic carbonate always volatilizes at the temperature required for completely burning the charcoal. The same takes place, as is well known, in many other cases in chemical analysis, but to a much greater extent than is generally assumed.

Ott (150) mentions some precautions in examining commercial carbolic acid. Crookes' method (dissolving the product in caustic soda and separating the indifferent oil floating at the top) is not applicable when the homologues of carbolic acid are present. In this case it is necessary to take the boiling point, which should lie between 187° and 197° C.

Mohr (173) gives an estimation of potash and soda by weighing them first as chlorides, then evaporating them with nitric acid, and weighing them as nitrates. The formula for calculating them from those weights is, if the sum of the chlorides be called Ch, and that of the nitrates Ni,

$$K\,Cl = 14{\cdot}83\ Ch - 10{\cdot}2\ Ni.$$
$$Na\,Cl = 10{\cdot}2\ Ni - 13{\cdot}83\ Ch.$$

With reference to this method, Mohr draws attention to the fact that Penny, working in the same direction, in 1839, found the then generally assumed atomic weights of some of the most important elements incorrect, and made a series of experiments to rectify them. The equivalents for O, Cl, N, K, Na and Ag which he found, coincide perfectly with the values which Stas has found during the last years in his celebrated researches. Mohr says that the British chemist must have worked with a truly stupendous accuracy, for which he has never got the full praise he deserved.

Reichards (184) discusses the examination of solid bodies for occluded gas. This very interesting paper is hardly capable of being abstracted. The same author, speaking of the very sensitive reaction for sulphites and hyposulphites by reducing them with zinc and

observing the action on lead paper, mentions that zinc, except when just remelted, is nearly always thinly coated with sulphide, which vitiates the reaction. He, therefore, recommends the use of aluminium wire. The reaction is quite unmistakeable when the proportion is $\frac{1}{300000}$.

Kaemmerer (221) describes an improved method for estimating the alkali-metals in organic salts. It is well known that the operation of calcining such salts cannot be continued till all the charcoal is burnt off, on account of the volatility of the alkaline carbonates. It is, therefore, necessary merely to carbonise the substances, wash the alkaline carbonates out, and estimate them as chlorides. This is, of course, a tedious operation, which is also a source of almost unavoidable losses. It might be simplified by adding sulphuric acid to the charred mass, which then burns quickly enough, but this cannot be done without loss by spurting. Rose has, therefore, recommended to add a small quantity of ammonic nitrate to the charred mass, which is then easily burnt, and to bring it into weighable shape by treating it with ammonic chloride or sulphate. Unfortunately this method is not applicable in most cases, because the mixture of alkaline nitrates, nitrites, and hydrates acts very strongly on the platinum of the crucible. Now, Kaemmerer reverses the process. He first chars the substance at a low heat, then he adds a few crystals of pure ammonic sulphate, and a few drops of water; he drives away the water and ammonic carbonate by heating the handle of the crucible cover, and then the ammonic sulphate by gently heating the crucible bottom. The charcoal is often quite destroyed at that stage; if not, a small quantity of ammonic nitrate is added, which only acts on the charcoals but not on the alkaline sulphates previously formed, and, therefore, does not injure the platinum crucible. The experimental results he quotes are extremely satisfactory.

Hager (267) gives a reagent for recognising the admixture of sugar or dextrin in glycerin. Pure glycerin, when diluted with water and boiled with ammonic molybdate, and a few drops of nitric acid, does not show any colour reaction; but when it contains sugar or dextrin, the boiling liquid takes a blue colour. It is best to take five drops of glycerin, 100 or 120 drops of distilled water, 0·03 to 0·04 grammes ammonic molybdate, and one drop pure nitric acid (25 per

cent.). The whole is boiled in a test tube for one and a half minutes. A trace of sugar or dextrin then causes a fine blue tint of the liquid. Sugar of milk does not show this reaction, nor gum arabic either; it may, therefore, serve to detect cane sugar in sugar of milk, and dextrin in gum arabic.

Schlosing (269) describes a very interesting method of organic ultimate analysis, which allows the estimation of C, H, and N in one operation. Bisscherexch (272) another method, estimating C, H, and and O in one operation. I must refer for these to the originals.

Mohr (285) draws attention to the fact that many crystalline silicates are only very incompletely attacked by fluorides, and that, therefore, when they are to be decomposed in this way, it is necessary to ignite them in a finely pulverised state in a covered platinum crucible, but not to the melting point. They are then, after cooling, easily attackable by fluorides.

Fresenius (304) shows that the differences in the analyses of superphosphates by different chemists can often be explained by a diversity in the mode of lixiviating the substance. He gives the results of five different ways of proceeding with this operation, and finally recommends again the process described in his handbook, viz., to triturate the superphosphate so far that the lumps are finely divided; but the coarser and harder pieces are not reduced to powder. The mass is then lixiviated by decantantion with cold water, till the washings show no more acid reaction. The same author proves again by experiments against various contradictions, the statement in his handbook, that in separating lime and magnesia by ammonic oxalate, the first precipitation does not give quite accurate results, since the calcic oxalate very easily retains some magnesia. It is, therefore, to be disolved in H Cl, and reprecipitated by N H_3 and ammonic oxalate.

Braun (345) describes a new reagent for nickel even in small quantities. It is potassic sulphocarbonate, which is coloured crimson-brown by salts of nickel, and pink when they are in very small quantities. The reagent is obtained by preparing $\frac{1}{4}$ litre pure potassa solution of 5 per cent. It is exactly halved, and one half is saturated with H_2S; then the other half is added, and besides 10 C C bisulphide of carbon. The mixture is digested in a flask with a cork and elbow glass tube for two days, and occasionally shaken up. The

resulting dark orange liquid is poured off the remaining CS_2 and kept in well stoppered bottles. The sensibility of the reagent is very great; it shows $\frac{1}{100}$ milligramme nickel in 1 CC without any doubt. Before the reagent is used the other metals ought to be removed as much as possible, especially cobalt, the latter by means of potassic nitrite.

Fleck (351) describes an improved mode of estimating the hardness of water. Its principle is that an alcoholic solution of pure olive oil soap, when added to a solution of calcic sulphate, containing some reddened litmus, turns the latter blue when all calcic sulphate is decomposed by the soap. This is a far better indicator than the subsiding of the froth. It is very easy, and in fact always done, to combine an estimation of the permanent hardness with that of the total hardness.

Richter (360) describes methods for the technical analysis of coal gas, which do not require the application of mercury, and are very quickly performed, as standard solutions of the reagents are employed. The paper is rather voluminous, and I must refer to the original; it is well worthy the attention of those who have to perform many analyses of coal gas, etc.

Schulze (390) has modified his well known process for the estimation of nitrates (measurement of the deficiency of hydrogen caused by nitrates in dissolving a known quantity of aluminium powder in an alkaline solution), so that it retains its accuracy in the presence of organic substance. He destroys the latter by potassic permanganates and the excess of the latter by formic acid.

Rieckter (400) has succeeded in detecting strychnine in the heart, lungs, and liver of an ox, by all the usual reagents, after 11 years exposure to decomposition.

Wimmel (411) shows that permanganic acid is not a reliable reagent for the detection of citric and tartaric acids, as Chapman and Smith had asserted.

Marx (412) describes a volumetric method for the estimation of nitric acid in natural waters, which is accurate enough to show half milligramme in 50 CC water. It is founded on the decolorisation of a standard solution of indigo by the water, heated by addition of concentrated sulphuric acid.

Nessler (415) refers to the fact that English chemists have proposed to use his ammonia test for quantitative determinations, and he remarks that, in order to obtain reliable results, it is necessary to have regard both to the temperature of the liquid and the quantity of fixed alkali in it. A very small variation in either of this causes a considerable difference in the colouration, even when the quantity of ammonia is the same in both cases. He remarks that his test will also separate the ammonia, so that it can be weighed as ammonic platinic chloride. For this purpose he adds to the liquid a large quantity of caustic potash or soda, then the potassic mercuric iodide, and digests it for twenty-four to forty-eight hours at a temperature of 37° C. Then the clear liquid is decanted, the precipitate is washed into a flask, caustic soda and sodic sulphide are added to it, and the ammonia is distilled off into hydrochloric acid, whence it can be precipitated by platinic chloride. Nitric acid can be estimated by first reducing it to ammonia; in concentrated solutions some of the latter is easily lost by being carried away with the hydrogen, but it can be recovered by conducting the H through water or H Cl, and adding this liquid to the original one.

Muck (416) gives practical hints on the estimation of sulphur, phosphorus, and copper in iron ores. He prefers to resolve the ore in the dry way, by igniting it with potassic nitrate and sodic carbonate. The fused mass is dissolved in water, and the sulphur precipitated from the solution; the filtrate contains some, but not always all, the phosphorus. The residue from the aqueous solution is dried, evaporated twice with H Cl, and then again with aqueous S O_2, till most of the ferric oxide is reduced. It is then treated with hot water, and the copper thrown down by H_2S; the filtrate, joined to the above-mentioned filtrate, serves for the estimation of phosphorus by Fresenius' method.

Gante (427) gives some more hints on the same subject.

Ondemans (419) gives tables of the solubility of crystallised magnesic sulphate, nitrate, and chloride, manganic nitrate, plumbic acetate, and zincic nitrate.

Gintl (422) describes two methods for the estimation of carbon in graphite. The first is to heat graphite with plumbic oxide in a glass tube, after having weighed the whole, until all the carbon is expelled,

as carbonic anhydride, and so calculate the carbon from the loss of weight. Or he fluxes the graphite with potassic nitrate, and estimates alkalimetrically how much potassic carbonate has been formed. Both plans gave very satisfactory results.

Schiff (430) describes a very simple apparatus for the collection of nitrogen in organic combustion, which saves the mercury trough and the manipulation with caustic potash.

Huschke (434) proves, against Siewers's contradiction, the reliability of Field's method for the estimation of chlorine, bromine, and iodine when found together. The liquid is divided in three equal parts, which are each of them precipitated by silver salt. Precipitate No. 1 is dried and weighed direct; No. 2 is digested with potassic bromide, which converts the argentic chloride into bromide, and No. 3 with potassic iodide which converts both the argentic chloride and bromide into iodide. The calculation is then easy.

Ullgren (442) describes improvements in the precipitation of copper by zinc, for which he uses a very simple galvanic apparatus.

Müller (465) describes a simple gas furnace.

Schönbein (486) makes some remarks on guaiacum as a reagent for active oxygen. He also shows that the ozone in turpentine can be transferred to acidulated water by shaking it with the latter.

Inette (489) employs the effect of tartaric and malic acids in preventing the precipitation of iron, aluminium, and manganese in alkaline liquids for the quantitative estimation of those organic acids.

Hahlschmidt (493) describes a method for estimating sugar in cane by a solution of potassic ferrocyanide in baryta water.

Mulder (507) treats of the analysis of mineral waters. He employs a temperature of 120° C for drying the evaporation residue, and shows, against Rose, that the magnesic carbonate does not lose any carbonic anhydride at that temperature. He also shows what source of inaccuracy is caused by the employment of glass vessels.

Schachtrupp (509) treats of the estimation of morphia.

Vogel (512) of the estimation of fatty matter in milk.

Haebler (513) of albumen in urine.

Huppert, Vogel, and Schmidt (517), of the difficulties in the volumetric estimation of uric acid in urine by the iodine method.

Riederer (517) of the examination for mercury in the animal organism by means of dialysis.

Dragendorff (521) gives a very long and interesting paper on the separation and estimation of alcaloids in forensic chemical examinations, which cannot very well be abridged.

Dr. LUNGE also read the following, abstracted from the Zeitschr:

ON THE ESTIMATION OF COPPER IN CUPREOUS SCHISTS.

(*Communicated by the Managing Board of the Mansfeldt Mining and Smelting Works, Vol. VIII., p.* 1.)

THIS board requires to have hundreds of copper assays made every month, for the purpose of deciding which of the workings are worth pursuing and which not. Rough tests are not sufficient for this purpose, and from this reason alone the dry method was altogether excluded from the competition. Up to the latest time the poor ores had been treated by Rose's method (precipitating by H_2S, igniting in a current of H, and weighing as Cu_2S), and the richer ores by the Swedish assay (precipitation by metallic iron). Both are exact enough if performed by skilled hands, but very lengthy and full of detail. It appeared especially desirable to do away with Rose's test for poor ores, as most of the samples belong to this class; the same circumstance, by the way, also applies to this district, where assays of poor ores have to be incessantly made. It is to be hoped that the exceedingly unfair dry copper assay will give way also in this country to the wet ones, now that the accuracy of the latter can be combined with the possibility of making many assays in a short time, as the following abstract from the paper in question will prove.

The above mentioned Mining Board, in May, 1867, issued a public invitation to compete for a prize for the description of a process for copper assays, to be given under the following conditions:—

1. Each assay must be finished in five to six hours inclusive of all by-work.

2. One man must be able to make at least eighteen assays in a day without any over-exertion.

3. The margin of difference must not exceed a fixed scale, ranging from $\frac{1}{10}$ in ores of 1% copper, to $\frac{1}{25}$ in ores of 6%, and so forth.

The price offered for a satisfactory solution was 300 Thaler (£45), and was to be awarded on June 30, 1868. In reply to this, sixteen different competitors sent in their papers, which were thoroughly sifted and tried by a scientific commission. This commission set aside all methods which presented any of the following disadvantages:—

1. Too many complicated and difficult operations.

2. Employment of varying quantities according to the greater or smaller per centage of copper in the ore (because in most cases this cannot be known beforehand).

3. Employment of expensive or explosive substances in roasting the samples.

4. Application of sodic hypo-sulphite and hydric-sulphide for the precipitation of copper (in order not to expose the assayers to deleterious gases).

5. Application of ammonia for the separation of copper from the earths and metallic oxides (because of its exceeding inaccuracy).

6. Too great a claim on the skill of the assayer.

7. The dry way altogether as inaccurate, expensive, and exhausting for the assayer.

Being sifted in this way, of all the sixteen methods only two stood the test, one of which could not receive the premium for formal reasons, but received an extra acknowledgment from the board.

These two methods are: the first by Dr. Steinbeck (precipitation by zinc, resolution and titration by potassic cyanide), the second by Mr. Luckow (precipitation by a galvanic current). Neither contains anything absolutely new, but this was not implied in the competition, and their methods are valuable because they combine well-known principles into very expeditious and accurate methods. Leaving out all unnecessary details, I shall give an exact description of both methods.

Steinbeck's method involves three operations: the extraction, precipitation, and estimation of the copper.

Five grammes of pulverized ore, whether rich or poor, in most cases unroasted, are put in a flask, and 40 to 50 CC crude hydrochloric

acid of 1·16 sp. gr. are added, whereby, of course, the carbonates are decomposed. Then the following quantities of a normal nitric acid (prepared by diluting pure acid of 1·2 sp. gr. with the same bulk of water) are added: 6 CC for bituminous ores, 1 CC for ores free from bitumens. (Very bituminous ores are first roasted and then only 1 CC nitric acid is used.) The samples are digested half-an-hour on a sand bath at a moderate temperature, then boiled for ten or fifteen minutes. The solution of the copper is quite perfect, and no trace of lower nitrogen oxides remains behind, which is very important. The solution is filtered into a covered beaker of a capacity of 400 CC, on the bottom of which there is a strip of platinum foil, and upon this a small zinc rod weighing about seven grammes. The zinc should not contain too much lead, but 0·1 to 0·3 % does not much matter. It is well-known how exceedingly rare it is to meet zinc quite free from lead, and just from this reason the simple precipitation by zinc is not an accurate method. The precipitation of the copper begins during the filtrations, and is very quick in the hot and concentrated solution, which is also free from nitric acid; it is greatly promoted by the galvanic action between the platinum and zinc. In half to three-quarters of an hour it is complete, which has to be confirmed by hydric-sulphide solution. The copper is separated from the excess of zinc and washed by decantation with ordinary clear water. The original paper points out the great advantages of this method over the ammonia, hydric-sulphide, and hypo-sulphite methods. To the copper in the beaker, which is partly loose and partly adheres to the platinum, there are added 8 CC of the normal nitric acid, and the solution is assisted by moderately heating. In the case of rich ores (over 6 %), which can be recognised by the bulk of the copper precipitate, 16 CC are taken. The solution is completely cooled, and immediately before titrating it 10 CC of a normal ammonia solution (made from 2 vols. water and 1 vol. ammonia of 0·93 sp. gr.) are added. If, in the case of rich ores, the double quantity of acid has been used, the solution is diluted up to 100 CC and 50 CC taken out with a pipette; each half receives the 10 CC normal ammonia and is tested by itself. Such ores, with more than 6 per cent. copper, are, however, very rare in this district as well as in ours. In this way the solution contains only *one* salt of ammonia, and always in the same quantity; how

important this is in the case of the potassic cyanide method is very well known, and a great many experiments are quoted to prove the satisfactory character of this way of proceeding. The standard solution of potassic cyanide is so adjusted that each CC shows 0·005 copper; as 5 grammes ore have been employed, each CC shows 0·1 per cent. of copper. The strength of the solution only changes in the course of several weeks, which does not at all matter in a laboratory where many assays are made, and a large quantity of test solutions is used. In this way 6 assays can be finished in 4 hours without any exertion, or 20 assays in two sets in the daily working time of 7½ hours, in a completely satisfactory way. It will be noticed that the zinc cannot, as in the ordinary ammonial solution, disturb the titration; the small quantity of lead is without any influence. One source gives experimental proofs for both. A very important fact is also this, that the titration is made *in the cold*, and it is shown what deviations are caused by working at a higher temperature.

Luckow's method, although it is very interesting, I must treat more shortly. He found that a small quantity of nitric acid (not exceeding 10%) in the solution does not at all interfere with the precipitation of copper by a galvanic current, and that nitric acid acts even better than sulphuric; but certain precautions are necessary in removing the substance out of the acid, not to leave the acid time to re-dissolve the copper. Two grammes of the ore are roasted in a porcelain crucible to destroy the bituminous matter, and, when cold, put in a beaker two inches high and 1½ inches wide. To this are added 4 CC of a mixture of 1 vol. concentrated sulphuric acid and 1 vol. water, then 6 CC nitric acid of 1·2 sp. gr., and 25 drops hyrochloric acid. The beaker is covered with a glass funnel the neck of which has been cut off; this completely prevents loss by spurting. It is then heated on a sand bath, first gently, then more strongly, until all the sulphuric acid has been driven off; this takes hardly over an hour, then the glass is cooled, the funnel washed with a mixture of 1 vol. nitric acid of 1·2 sp. gr. and 6 vols. water, and the beaker half filled with it; a few drops of a concentrated solution of tartaric acid are also added. Then a spiral of thick platinum wire is immersed in it, resting on the bottom of the beaker, and a piece of platinum foil bent in the shape of a cylinder and fastened to an adjustable stand is lowered in the glass, till it very nearly reaches

the platinum spiral, but it must not touch it in any point. The spiral is connected with the positive, the cylinder with the negative pole of a simple galvanic battery. All the copper is precipitated in a coherent shape on the platinum foil, which has been weighed beforehand. After a time, mostly varying from 3 to 6, and never exceeding 8 hours, the precipitation is complete. Then the platinum cylinder is taken out of the glass without opening the battery, washed quickly with hot water, then with spirits of wine, dried for a few minutes in a water bath, and weighed again on a fine balance. The copper is in this way estimated correctly.

The galvanic battery which Luckow employs has been constructed by Mr. Koüger, and it consists of the following parts:—A glass beaker or tumbler 6″ high and 4″ wide, within this a hollow lead cylinder $6\frac{1}{2}$″ high and 2″ wide, with 4 openings at the bottom, and a zinc cylinder $3\frac{1}{2}$″ high and $2\frac{3}{4}$″ wide, which hangs from the top of the beaker on 3 copper wires, so that its bottom is 3″ from the bottom of the beaker. The beaker is filled to three-fourths of its height with rain water, in which $2\frac{1}{2}$ oz. Epsom salts are dissolved; then, first the zinc and afterwards the lead cylinder are introduced, and the whole is put in a quiet place where it is not liable to be shaken or otherwise disturbed. Here the elements are combined into batteries of 4 or 6 each. When they are to be used, a hand-full of blue copperas is thrown into the lead cylinder, and the current is closed when the copper solution reaches the top of the openings in the bottom of the lead cylinder. Such a battery goes on well for $2\frac{1}{2}$ or 3 months, and can be set going again for the time by siphoning off half the liquid contents and replacing them by fresh water. A battery of 4 cells precipitates on an average 0·100 grammes copper from a concentrated, but less from a diluted, solution.

One man can, with 12 batteries, make 24 assays in a day, as they can be left to work over night.

On the motion of the CHAIRMAN the thanks of the meeting were given to Dr. Lunge.

Mr. J. W. SWAN exhibited a Holz Induction Machine.

Newcastle-upon-Tyne Chemical Society.

FIFTH GENERAL MEETING, APRIL 22ND, 1869.

ISAAC LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

Two new members were elected; the name of a candidate for admission was read for the first time; after which,

Dr. LUNGE read

A SUMMARY OF THE CONTENTS OF THE ANNALES DES MINES FOR 1868, AS FAR AS INTERESTING TO CHEMISTS.

DAUBRÉE's "Synthetical Experiments relating to Metéorites."—A very lengthy and important paper. The same, "On Some New Beds of Calcic Phosphate," treats of the new discoveries of such beds in all countries, and is very important to agriculturists.

"General Report on the State of the French Empire in 1867."—Contains the most varied and interesting information, especially on exports and imports, with many statistical tables; also on the mineral industry of France.

Review and Complete Translation of the Prussian Codified Law of Mines.—Freycinet's Supplementary Report on the Sanitary Improvements in Towns and Factories in France and Foreign Countries.—A great part of this lengthy paper treats of the effect of the English Factory and Alkali Acts. In speaking of the insalubrious effects of working with lead and its products, the author gives a most interesting description of Ozonf's method of manufacturing white lead, with many

copper-plates. Ozonf's process, as is well-known, bases on the old French method of wet precipitation by carbonic anhydride, but his peculiarity is the employment of pure gas, obtained by absorbing the carbonic anhydride in a solution of sodic carbonate, and boiling the latter till it has lost the absorbed gas again. Another important chapter is that on lucifer matches. But the most important one for our locality is an abstract from Dr. Smith's Report on the Alkali Act, accompanied by Freycinet's remarks. He certainly makes some important mistakes, for instance, by asserting that experience has again proved that complete condensation could only be hoped for by muffle furnaces (blind roasters). The Tyneside alkali manufacturers will learn more from his report on sulphuretted hydrogen where he criticizes the method of treating the tank waste at Dieuze by Hofmann's process. He describes the apparatus which is there employed for burning H^2S, but which acts incompletely and is hardly applicable at all on a larger scale, especially owing to the explosions it gives rise to.

Later on there is a description of the whole process of utilizing the tank waste and still liquor as carried on at Dieuze, with plates. The pollution of streams is also treated at great length, as well as many other matters of the greatest public importance, but it is quite impossible to render justice to this treatise of 180 octavo pages in a meagre abstract like this.

Grüner on the Present State of the Metallurgy of Lead.—It is impossible to give here an idea of the importance of this treatise (of 82 pages, with many plates), which must be called classical, and is worthy of the most attentive study of all those who are interested in lead smelting. I will only mention, therefore, that he treats at great length, and with the fullest details of practical working experience, of the zinc process for desilverizing lead, which, according to his statements, seems destined to supersede Pattinson's process.

A Report of the Mining and Metallurgical Industry of Sweden will have great interest for a special circle.

A paper by Billy, on the Invention of Bessemer's Process for the Manufacture of Steel, does not give much new practical information, but is of great historical interest.

The PRESIDENT said, he had completely failed in repeated

attempts to utilize hydro-sulphuric acid in chambers. Apart from the mere difficulty of effecting its complete combustion, it was very difficult to obtain with it any regularity of working.

Mr. GLOVER said, that in the experiments referred to by the President, one chamber working with hydro-sulphuric acid only, yielded rather less oil of vitriol than was consumed in the nitre-pots.

Mr. HENRY BOWMAN read a translation of a pamphlet by Mons. CARON

UPON THE USE OF MAGNESIA AS A HEAT-RESISTING MATERIAL.

AFTER some introductory remarks upon the price and use (Comptes rendus LXII., 296) of magnesia, which can hardly be of interest to this society, Mons. Caron goes on to describe the process by which he makes magnesia into highly refractory crucibles and bricks, a translation of which I have thought might be of use to some of the members, and so subjoin :—

"The magnesia which I have hitherto used comes from the island Eubeus, where it is found in considerable quantity in the form of a white carbonate, very compact and pretty hard. This carbonate contains traces of lime, silica, and iron, besides which it is veined with thick plates of silica and serpentine-like matter, which diminish its infusibility and render it unfit for the oxy-hydrogen light if they be not removed. These plates are, however, very easily known, and will be removed even from large quantities without difficulty, and in bricks their presence will give rise to no greater inconvenience than a slight vitrification at very high temperatures, unattended by any serious consequences. Before crushing the carbonate it is well to calcine it at a temperature high enough to drive off the carbonic acid; the matter is then very friable, and easily ground. At this stage it is easy to separate the silica and serpentine which do not fall by the action of the heat. This preliminary treatment does not, however, allow of agglomerating the magnesia, and even supposing this difficulty overcome, a heat much greater than that at which it was calcined would cause a great

contraction, and so crack and twist the matter as to make it useless. Before moulding the magnesia then, it is absolutely necessary to submit it to a heat at least equal to that which it will have to bear afterwards. Calcined in this way it is not yet plastic; it has a sandy appearance, and pressure does not give it the smallest power of cohesion; a mixture of a small quantity of less calcined magnesia gives it this quality. The quantity of the latter necessarily varies with the degree to which the two magnesias are calcined; it is about one-sixth of the weight of that which has been submitted to the highest temperature (steel melting). It is, however, to be borne in mind that the smallest possible quantity, compatible with a good power of cohesion, has to be used. There remains then but to wet it with 10 to 15 per cent. of water, and press it heavily in cast iron moulds, similar to those used in making patent fuel. The brick thus produced becomes hard in drying, and harder still when it is baked at a red heat. It would seem that the same process might be followed in making large crucibles, but it is difficult to press either large quantities or masses with great surfaces, for the magnesia sticks tightly to the sides of the mould. Successful as I have been in making small crucibles in this way for use in the laboratory, still I do not look upon it as applicable to making very large ones for melting steel, &c. It is better in this and some other cases to agglomerate the magnesia by the wet way. To make magnesia in some sort plastic, I have profited by a property of this earth mentioned by Berzelius in his chemistry. Strongly calcined and moistened it hardens in drying. This fact is due, no doubt, to a combination with the water which is not pointed out by any notable rise in temperature. I have remarked besides that the magnesia when solidified in this way parts with the water at a very high temperature only; thus the baking not only does not destroy its powers of cohesion, but on the contrary gives it a hardness and strength equal to ordinary crucibles. This being the case it is easy to see the use which can be made of this property. Thus, the magnesia ready calcined has only to be wetted and moulded, and then dried and baked. In order to repair steel-melting furnaces, it is only necessary to plaster the sides with the magnesia paste, which will bake when the furnaces are lighted without any further care. Sometimes, however, whether it be because the magnesia has been too much or too little heated, or because it

contained siliceous matter, the crucibles either before or after baking are not so strong as could be wished. In this case it is necessary to dip them into a cold saturated solution of boracic acid, dry and bake them as before. This does not make the magnesia more fusible, it simply makes the atoms cohere more strongly. Very pure magnesia, strongly calcined and very finely ground, could be used in the form of a thin paste to make the thinnest and most delicate crucibles, as well as the most beautiful and complicated casts, etc. I am sure that in a short time this earth will be profitably used in pottery, notwithstanding the difficulty of moulding it as compared with clay."

Mr. BOWMAN mentioned, that he had experienced considerable difficulty in preparing the bricks from their adhesion to the mould. He had been more successful with some cupels which he exhibited. One of these had been immersed in melted soda for some hours, and when taken out was perfectly firm and coherent though saturated with soda. Another (exhibited) had been used as a support in melting platinum and was perfectly unaffected, though it had cracked in two or three places. Mr. Bowman attributed this to insufficient heating during its preparation.

The PRESIDENT observed, that any improvement in materials for resisting high temperatures was of interest in face of the recent development of our means for producing these latter.

Mr. B. S. PROCTOR read the following

NOTE ON RHUBARB ROOT.

THE common custom in judging the quality of articles of vegetable materia medica is to estimate their value by their physical or sensible properties—not unfrequently by their appearance only.

It does not require more than a moment's consideration to see how fallacious such criteria must frequently be, especially if we bear in mind that beauty of appearance, brightness of colour, or paleness of colour are apt to be considered recommendations, without considering how far these appearances coincide with a good proportion or good condition of the active constituents.

There are probably few more conspicuous instances of this want

of intelligent principle in the common practice than in the case of medicinal rhubarb.

Roughly stated, rhubarb may be said to consist of extractive matter soluble in alcohol, in which the medicinal properties reside, mucilaginous matter soluble in water, vegetable tissue, and salts consisting mainly of oxalate of lime.

The active matter being dark yellow brown, and the inert matter, especially the oxalate of lime, being white, we might naturally say that paleness of colour should not be *prima facie* a recommendation, though such is the rule of custom with regard to powdered rhubarb.

In the commerce of the crude root the custom is not more satisfactory.

It is well known that the active constituents of plants are usually most abundant in the cortical portion, whether it be of the root or stem, and this knowledge should have deterred the dealer from attempting to improve the quality by rasping off the outside, but common sense has not yet proved strong enough to overcome this old custom, or I should not now have to draw your attention to a comparative examination of the cortical and central portions of the root, which has for its object the correction of this ill-judged custom.

For experiment I selected a specimen of root of so-called common quality, free from decay, but not improved by the common practice of trimming off the outside. Having split it and scooped out from one-half so much of the inside as was palpably softer than the outside, the inner and outer portions were separately reduced to fine powder, and are spoken of below as central and cortical rhubarb, the other half of the root was reserved and is now on the table.

Long continued trituration in a mortar did not produce the well-known yellow colour of commercial powdered rhubarb, but on rubbing the powder with a little water, again drying and powdering it, the colour was developed in the central rhubarb, though even this treatment did not produce a bright colour in the cortical portion, as the two samples will show.

To estimate the relative composition of these two powders 100 grains of each were taken, and the matter soluble in rectified spirit extracted by maceration in successive small portions till exhaustion was approximately, though of course not absolutely, effected.

On evaporating the two tinctures the cortical rhubarb was found

to have yielded thirty-six grains of extract, and the central only thirty. The marc was in each case treated with H Cl and spirit, and washed with proof spirit to remove earthy matter, &c.; when dried, that of the cortical rhubarb weighed forty-eight grains, that of the central fifty-three, giving by deduction the portion soluble in H Cl and spirit as—

In the cortical	16·0
In the central	17·0

The marcs of these last operations were then beaten up with an 8% solution of hydrate of potass, macerated for a day, diluted with water, thrown on a filter, and washed with water continuously for a week, the marcs being again dried and weighed gave by deduction the matter soluble in caustic alkali and water as—

Cortical	26·5
Central	26·0

And the residues, which we may regard as vegetable tissues, weighed,

Of the cortical	21·5
Of the central	27·0

In the above examination there was evidently a larger quantity of organic matter dissolved by the H Cl and spirit, the washings from the cortical portion, more especially, being highly coloured; a second examination was, consequently, undertaken to ascertain the quantity of ash yielded by the respective parts of the root, and the quantity of extract obtained from each by the use of the mixture of spirit and water directed by the pharmacopœia to be used in the preparation of medicinal extract of rhubarb, that is one part of rectified spirit and ten of water. The extraction was conducted as before, with a yield of

49·4 grains from the cortical, and
37·0 „ from the central rhubarb.

The marcs weighed

53 grains from the cortical,
60 „ from the central,

and when burnt left ashes weighing from

Cortical	5·4
Central	12·0 grains,

The ashes in both cases consisting principally of carbonate of lime, with small portions of magnesia, alumina, and iron. Supposing the carbonate of lime found to represent oxalate of lime in the root as generally stated, the proportion in the above cases would be about as follows :—

Oxalate of lime	in cortical	...	... 8·4 %
Do.	in central	...	... 18·0 %

and a corresponding deduction should be taken from the weight set down as representing vegetable tissue. Having obtained so striking a difference in the quantity of ash from two parts of the same root, it became desirable to know whether the extreme outside would give any farther difference in this respect, and to determine the point a sample was cut off the outside of the reserved half of the root and burnt off; it left 9·6% of ash, of which 3·4 was soluble in water, and the remainder in H Cl. As this indicated 6·2 of insoluble ash against 5·4 obtained in the former case, it appeared desirable to ascertain whether or not any earthy salts had been taken up with the extract.

Ten grains of the alcoholic extract left an ash, of which the insoluble portion was too small to be weighed with the appliances at my command, but in which lime, alumina, and iron could be traced. Ten grains of the extract, obtained by spirit and water, gave an ash which stated on per centages of the root was—

Soluble in water	...	...	...	3·3
Insoluble in water	...	...	...	1·1
Total	...	...	...	4·4

The soluble portion consisting of carbonate sulphate and chloride of potassium, the insoluble of lime, alumina, and oxide of iron. If this 1·1 be added to the 5·4 of insoluble ash obtained from the marc, it shows, as was anticipated, a larger, though only slightly larger proportion of lime salts in this cortical part than in the extreme outside of the same.

From these results, I think, we may conclude that the custom of trimming off the dark outside of the root for the sake of improving its appearance is a practice which ought not to be countenanced, and that in judging the quality of powdered rhubarb of commerce, the brightness of the colour, or paleness of colour, which may indicate

the absence of decayed roots may also indicate low per centage of the active constituents and a high per centage of oxalate of lime.

RHUBARB ROOT.

	CORTICAL.	CENTRAL.
Extracted by rectified spirit	36·0	30·0
Do. H Cl and proof spirit	16·0	17·0
Do. KO and water	26·5	26·0
Insoluble residue	21·5	27·0
	100·0	100·0
Extracted by mixture of rectified spirit 1 part, water 10 parts	49·4	37·0
Marc, consisting of combustible matter	47·6	48·0
Ash	5·4	12·0
	102·4	97·0

Looking over those rough analytical results the first thing that strikes us is the discrepancy in the second table between the weight of the body analysed and the sum of the bodies found, but in this respect I have not been more unfortunate than others who have walked the same path before me. In Brande's analysis there is a loss of 8·2 per cent., and from this the loss by various analysts varies down to that of Horniman, which is 0·9 per cent., and I suspect, if we knew the whole circumstances of the case, Horniman's small loss might turn out to be no small gain, for he gives 14·0 per cent. as the weight of fibre, including the insoluble salts, &c.; and as he gives 5·88 of carbonate of lime obtained by the incineration of this fibre, which would give about 9 per cent. of oxalate, which, together with the silica-magnesia and alumina, amounting to 2·8, being deducted from the 14 per cent., leaves the incredibly small per centage of 2·2 as proportion of vegetable tissue in the root. The difficulty of getting hygroscopic vegetable substances absolutely dry without decomposition is no doubt the cause of these discrepancies. I endeavoured to dry down the extract from cortical rhubarb so as to give 100 per cent. as the sum of the constituents, and found that by a temperature a little over 212° there was a further reduction in the weight, making the sum come to less than 100, but the extract had ceased to be entirely soluble.

The extraordinary variation in the quantity of oxalate of lime found by various analysts may also be noted, and in some measure explained, if we suppose that different parts of the root were selected for examination. Mr. Queckett is said to have obtained 35 to 40 grains of oxalate of lime raphides from 100 grains of rhubarb. He no doubt selected the soft interior of the root as most readily yielding the raphides in a condition fit for microscopic examination, while other analysts have employed the whole root, or it may be, the external part only.

The thanks of the meeting were given to the authors of the papers.

Newcastle-upon-Tyne Chemical Society.

SPECIAL MEETING, MAY 6TH, 1869.

MR. J. GLOVER IN THE CHAIR.

Mr. LUDWIG MOND read the following

ON THE RECOVERY OF SULPHUR FROM ALKALI WASTE.

IN endeavouring to entertain you to-night on the subject of alkali waste, I am afraid that I shall not be able to afford you as pleasant an evening as you have been accustomed to during the short existence of this society by the various most valuable papers that have come before you. I certainly could not have chosen a more awkward subject, a subject which will rouse in many of you the most disagreeable feelings and notions. I see here several gentlemen who have spent thousands of pounds in trying to turn this obstinate substance to advantage. I see many more who still spend many hundreds every year to have it carried far away to the dumb fishes of the sea who cannot petition Local Boards or Parliament nor bring a suit for nuisance, and probably there is nobody in this room who has not more than once had his nose offended and his appetite spoiled by coming in too close a contact with the waste heaps in this neighbourhood. However, as I believe myself to have been more successful than anybody else in dealing with the waste on a manufacturing scale, I will try what I can do with it on paper. But in doing so I must appeal to your kindness and indulgence on various accounts. Firstly, because I am a foreigner and

have not your language at my command; secondly, because I have to-night very little to bring before you which has not been already published; and thirdly, because I feel that addressing so many manufacturers, matter of fact men, I should give more prominence than usual to the details of plant and working, and to actual figures and results obtained, which may, to many amongst you, appear uninteresting. Very soon after I had become first practically acquainted with the alkali manufacture at the works at Ringkuhl, near Cassel, in Germany, my attention was drawn to the waste, and, already, in 1860, I began a series of experiments on several older processes for the recovery of sulphur, particularly the brilliant proposals of your most distinguished countryman, Mr. Wm. Gossage. While so engaged the manager of the above-named works, Dr. Guckelberger, requested me to make an experiment on the manufacture of hyposulphite of soda from the waste by the process of your fellow-townsman, Mr. William S. Losh, who took out a patent in 1852 for obtaining salts of soda from the waste, which yields very fine hyposulphite, and which, under the able management of Mr. Calvert Clapham, is still most successfully worked. But, unfortunately, this process only utilizes the sulphur in the waste to a small extent, and, consequently, does not prevent the nuisance arising from it. In the course of my experiments on this process I obtained, to our great surprise, in place of a small quantity of hyposulphite, as we had expected, a quantity equal to one-fifth of the weight of the waste employed, thus representing one-fourth of the sulphur contained in the waste; and on examining the waste after this hyposulphite had been separated by lixiviation, I found that the waste was not yet fully oxidised, but still contained large quantities of sulphides. This at once suggested to me the idea that it might be possible, with careful management, to separate by a simple oxidation and lixiviation a quantity of soluble sulphur compounds from the waste, which would be large enough to make the recovery of sulphur by the decomposition of these compounds with muriatic acid a very feasible and well-paying process. I proposed this idea to Dr. Guckelberger, who, though at first not favourably inclined towards it, allowed me with the greatest liberality to go into extensive experiments on the subject, and gave me the benefit of his valuable advice. I soon ascertained that on exposing

the waste to the air the soluble sulphur compounds would only increase up to a certain maximum, but will decrease again when the exposure is allowed to continue. If, however, these compounds are removed at the proper time by lixiviation, the waste will, on renewed exposure to the air, yield a second and even a third time a considerable quantity of soluble compounds almost equal to the quantity obtained at first. In the laboratory where I exposed the waste to the air in shallow dishes, I thus arrived very soon at obtaining as much as 60 per cent. of the sulphur contained in the waste. But when I started working on the large scale, exposing the waste in heaps of various sizes, I obtained, in spite of all my efforts, only very small quantities of sulphur. So I left Ringkuhl in 1861 without having brought the matter to a satisfactory issue. I continued my experiments with waste from a Rhenish alkali works, and tried to oxidise it by forcing air through it, but in this I also completely failed. At the time mentioned many of the Continental works still lixiviated the black ash by a process during which it was removed from one vat to the other. In consequence thereof the waste was obtained so dense that it was impossible to get a sufficient quantity of air through any mass of it within a reasonable time, and I had at last to expose it to the air in shallow layers on large hurdles in order to obtain the desired result on the large scale. Sulphur being very dear at this time, and labour in my native country very cheap, I considered this process sufficiently feasible, and patented it in December, 1861. In the beginning of 1862 it was, at my request, tried on a small scale at Ringkuhl, and with perfectly satisfactory results. When I came to England, at the time of the great Exhibition, I found, to my great surprise, that the chemical reactions involved in this process which I had believed to be new and original had already been proposed for the same purpose as early as 1836 by Mr. Thomas Hudson Leighton, one of the first alkali manufacturers in this district, and that at the same time with myself three different parties had been working in the same direction in three different corners of this country. Messrs. Townsend and Walker in Glasgow, Mr. J. L. Jullion in this town, and Mr. Alfred Noble in Bristol. Mr. Leighton had already proposed to allow the waste to remain in the tanks until it heats, then to lixiviate with water and to precipitate the sulphur from the liquor so

obtained by muriatic acid. None of the other gentlemen above-named carried the matter further as far as sulphur recovery is concerned. All of them oxidised the waste only once, either in tanks, as Mr. Leighton, or in heaps, as Mr. Losh had proposed, and, consequently, they could but obtain small quantities of sulphur. whilst they left the waste quite as much a nuisance as it was before. My process thus clearly had the merit of being the first which yielded reasonable quantities of sulphur, and did away with the nuisance arising from the waste, but unhappily I was soon convinced that it was a great deal too expensive in this country, where I found land and labour fully three times as dear as in the place I came from. I also observed the great difference between the waste I had formerly worked upon and the English waste, which being obtained by the excellent and most rational method of lixiviation now generally in use is much more porous. I had very little doubt that the latter would allow of a much simpler and cheaper treatment. I induced the late Mr. John Hutchinson, of Widnes, to take the matter up and to enable me to work out on a very reasonable scale my old favourite idea, viz., by forcing air through it. I tried at once to treat the waste in the same vessel in which it had been made, so that the sulphur might be extracted without any removal of the waste, and was this time completely successful. I took out a patent for this new process in September, 1863, and at this time I had already treated a number of tanks containing 10 tons of waste each, and had obtained as much as 55 per cent. of the sulphur which it contained. I had to leave England shortly afterwards in order to fulfil an engagement for the establishment of a new alkali works in Holland. Unfortunately the health of Mr. Hutchinson, who had taken a great interest in my process, at this time became so bad that he had almost entirely to abstain from business, and his subsequent early death prevented, for several years, anything further being done in the matter at his works. So it happened that the process was first carried out on a manufacturing scale in autumn, 1864, at the new works of Messrs. Smits and De Wolff, at Utrecht, where I had in the meantime got to work. Here the process succeeded remarkably well from the very first day, and actually gave me less trouble than any other branch of the alkali manufacture, and it has ever since been working most regularly and

satisfactorily. After some years the trustees of Mr. Hutchinson took the matter up again, and though they at first met with some difficulties, these were so completely overcome that plant for the working of the waste from 300 tons of salt-cake a-week is now in course of erection at these works. At the beginning of last year Messrs. Charles Tennant and Co. introduced my process with the most perfect success in their works at Glasgow, and since it has been adopted with equally satisfactory results by Messrs. James Muspratt and Sons, in Widnes, and the Netham Chemical Co. in Bristol. The first firm who introduced the process in this district was the Tyne Chemical Co., whose works are under the able management of your learned secretary, my esteemed friend, Dr. G. Lunge. Having started only a few days ago I cannot, of course, yet speak of the results obtained here, but I am glad to be able to state already that the waste of this district appears to be even more suitable for the treatment by my process than the Lancashire waste, as it oxidises very readily and with a comparatively small pressure of blast. I have drawn up a rough sketch of the plant necessary for the working of the process, to which I now wish to call your attention. In addition to the four tanks usually employed for the lixiviation of black ash you find here six tanks more, thus forming a set of 10, all of which are connected in the usual way by pipes and stoppers. They are fitted up as usual with taps and spouts, but also with two additional spouts for the conveyance of sulphur liquors, and with two spouts with holes and plugs running over the top of the tanks. All the tanks are also provided with perforated false bottoms, and the space between the two bottoms is connected to a common smithy fan by pipes which stand on the top of the false bottoms, and which can be opened or shut by dampers. I usually employ straight cast iron pipes of three inches diameter, which carry a main windpipe of eight inches diameter, made of galvanised iron, and running right over the top of the tanks. The fan may be of very moderate dimensions, a smithy fan of Schiele's construction of twenty inches diameter, which can be driven by a two-horse power engine, and costs only eight pounds, being sufficient for the treatment of the waste from 100 tons of saltcake per week. The taps of the tanks are so arranged that by a short piece of spout, or better, a piece of India rubber tubing, they can run into either of the two spouts underneath them.

These spouts run to different wells, from whence the stronger liquors are pumped into settlers, while the weaker ones are also raised by pumps and run back into the tanks through the spouts running over the top. I keep these top spouts outside of the tanks, and lead the liquor into the tanks by a separate, short moveable piece of spout, so that in case of accident the sulphur liquor cannot get into the tanks filled with black ash, but falls to the ground. I prefer this arrangement to the pipes and cocks I previously used for connecting the tanks in such a way as to allow the sulphur liquor, as well as the soda liquor, to circulate without pumping, though I made these pipes work very satisfactorily where they have been worked with care and attention. The expense of the pumping is very trifling, and out of proportion to the loss which might be caused by any intermixture of the two liquors occasioned by accident or negligence. Three of the ten tanks always contain black ash in the ordinary course of lixiviation; one of them is either casting or filling, and the remaining six are full of waste in the different stages of my process. As soon as the soda is washed out, and the last weak liquor drained off the waste, the damper in the air pipe is drawn. The air which is delivered by the fan, with a pressure varying in different works from two to six inches of water, according to the density of the waste, penetrates the waste quite easily and very regularly, and soon causes it to heat and give off steam, the temperature rising within a few hours to 200 degrees Fahrenheit. This treatment is continued for 14 to 18 hours, after which time the largest possible quantity of soluble sulphur compounds will have been formed. The damper in the air-pipe is then shut, and the weaker liquors from another tank are pumped upon the waste until it is covered. The first liquor running out of a tank is usually sixteen to twenty degrees Twaddell, and all the liquor down to ten degrees Twaddell is strong enough for our purpose, and is run off and pumped into the sulphur liquor settlers. The waste is, at the same time, kept covered with weaker liquors and subsequently with water. The liquor under ten Twaddell is run into a second well and pumped into the next tank, which has, in the meantime, been sufficiently oxidised. When it comes down to five Twaddell the water is stopped running into the tank, and the liquor which remains in it is run through a separate spout into a third well, and is also pumped

up into the next tank and run through the second top spout. Consequently, the sulphur liquor is always run through two tanks, and is thus obtained fifteen to eighteen degrees Twaddell strong, and contains about half a pound of sulphur per gallon. This lixiviation is easily performed in eight to ten hours; as soon as it is finished the damper in the air-pipe is drawn, again the air is forced through the waste a second time in the same way as before. After the same number of hours the waste is again lixiviated in exactly the same manner as before described, and, subsequently, this treatment is repeated a third time. The strong liquors yielded by these different operations are pumped into the same settler. The average liquor so obtained should be of a certain composition, of which I will speak presently, and which depends on the proper regulation of the quantity of air forced through the waste. This regulation is effected by means of the dampers in the air-pipes, and with some experience the appearance of the waste and the way in which it is heating are sufficient to guide the workmen as far as is practically necessary. As a check the liquor is also daily analyzed, and more or less air given, according to the results of the test. The liquor contains principally hyposulphite of lime, bisulphide of calcium, and sulphydrate of calcium. It ought to contain at least one equivalent of $Ca\,OS_3\,O_2$ for every two of $Ca\,S_2$, and one of $Ca\,OS_2\,O_2$ for every one of Ca SHS. A certain quantity of the liquor is tested with a standard solution of iodine and starch, a second equal quantity is precipitated by a solution of acetate of zinc, and, after filtering, is also tested by the same solution of iodine. If the liquor has exactly the composition above-named, the iodine used for the second test is just one-fifth of the iodine used for the first test. It is, however, preferable to have a small excess of hyposulphite, because it is then more easy to separate the sulphur without the evolution of sulphuretted hydrogen, and in practice it is quite sufficient if the proportion of the first to the second test is kept between four to one and five to one. In order to avoid inconvenience I prefer to use two good sized settlers, so that there is always a stock of good liquor on hand, and that any bad liquor which might be made by accident or carelessness can be remedied by the addition of fresh liquor before it is used. The sulphur is separated from these liquors by running them together with an equivalent quantity of

muriatic acid, which may be used as weak as four to five degrees Twaddell, into a large round wooden vessel of ten to twelve feet diameter and three to six feet in height. At the same time steam is introduced into this vessel so as to keep the mixing liquids continually at a temperature of 140 degrees to 150 degrees Fahrenheit. The quantity of acid required can be very easily judged and regulated by (after having it well stirred) taking from time to time a sample of the liquid in the vessel, which ought to be white but turns grey on the addition of a small quantity of sulphur liquor. I prefer to use an agitator moved by machinery, which not only saves labour but also effects a more complete decomposition. When the vessel is full its contents are run by a stophole into a square wooden tank below provided with a perforated false bottom. Here the sulphur settles very rapidly and the clear supernatant liquor is then run away. In the meantime the round vessel has been filled again and is then again emptied into the tank below, and when a sufficient quantity of sulphur has there accumulated it is emptied into a similar tank standing beside the first one. Or the round vessel being once filled the liquor and acid may be run into it continually, and the mixed liquids, with the suspended sulphur, may be run continually into the square tank below, and from that, by a connection underneath the false bottom, into the second square tank. Almost all the sulphur will thus remain in the first tank, and the liquid will run completely clear from the top of the second tank. This clear liquid is a neutral solution of chloride of calcium containing only small quantities of sulphate and polythionate of lime, and, consequently, harmless. The sulphur is washed with water in these same tanks and is then raked out through a door at the bottom into drainers below, constructed of bricks and tiles, which are heated underneath. From these the sulphur is put into an iron pot or boat pan where the water which it still contains is boiled off and the sulphur melted. When completely melted it is run by a tap at the bottom of the pan into a brick bed, and after cooling is broken into pieces of convenient size, being thus obtained in the lumps that are before you, and containing only a few tenths of a per cent. of impurities, thus far surpassing any brimstone imported from Sicily. It contains, however, small traces of arsenic derived from the muriatic acid employed. Though this is of no consequence

for most purposes where brimstone is employed, and has even no effect on the manufacture of pure sulphuric acid, which can be obtained absolutely free from arsenic with recovered sulphur, it may be easily avoided by either using acid obtained from roaster gases only which have been kept rigidly separated from the pot gases, or by precipitating the arsenic from the muriatic acid by adding a very small quantity of sulphur liquor to it and settling it before it is used. In those works where all the muriatic acid is already profitably employed, which, however, is very seldom the case in England, the acid waste liquors from the bleaching powder stills may be employed instead. The separation of the sulphur is carried out in exactly the same way as in using acid, but the sulphur obtained is of a dark colour and contains three to five per cent. impurities. The still liquor is thus converted into a completely colourless clear liquid, containing an almost absolutely neutral solution of chlorides of manganese and iron, and is, consequently, quite harmless. A second waste and offensive product may thus be advantageously got rid of at the same time the waste is deprived of its noxious properties. The waste after having undergone the described treatment has only small quantities of sulphide of calcium left in it, which are so enveloped by other compounds that the air can gain no access to it, and which, consequently, can no more give rise to the evolution of sulphuretted hydrogen or to the formation of yellow liquors if the waste is put up in heaps. The principal constituents of the waste are now carbonate of lime, caustic lime, and sulphate and sulphite of lime, all of which are very valuable manures for many soils. I have also lately tried to use this waste in ball-making in place of a portion of limestone or chalk, and though I cannot yet speak with confidence about the result, I have already obtained very fair black ash, and expect to settle this matter completely within a few months. Experiments carried on at Messrs. John Hutchinson and Co.'s works to the extent of about 100 balls with a mixture of 3 cwts. of wet waste and $1\frac{3}{4}$ cwt. of limestone to 3 cwts. of salt-cake have yielded a black ash only a very little inferior to the ordinary product, so that with more experience we expect to arrive soon at the desired end. If this process of sulphur recovery has been properly carried out, the quantity of sulphur obtained amounts to fully one-half of the sulphur contained

in the waste. I have even occasionally obtained as much as sixty per cent., and I have no doubt that in this manufacture, as in every other, the yield will gradually increase from year to year as it becomes better understood and as the workmen get more used to it. In order to remain on the safe side we will, however, only calculate on forty per cent. of the weight of the sulphur in the salt-cake used in making the balls, which can be obtained with comparative ease. A works decomposing 200 tons of salt-cake a-week would thus produce 800 tons of sulphur a-year. This sulphur sells, at this moment, at the price of £7 per ton; taking it, however, only at £6, a price which can, in all probability, be relied upon to be under the average of many years to come, it would realise £4,800 a-year. As you will easily be able to judge yourselves the cost of recovering this sulphur must be comparatively small. During the manufacture manual labour is almost completely avoided, and the only other expense is for fuel used in producing steam and in melting the sulphur. The actual cost per ton amounted, in Messrs. Hutchinson's works, during the quarter ending 31st March last :—

Wages	16s. 5d.
Fuel	4s. 6d.
Maintenance	4s. 6d.
Total	25s. 5d.

As the total production of sulphur during that quarter amounted, in consequence of some alterations which were being made, to only about three tons a-week, you will agree that working on a larger scale the cost will certainly remain under £1 per ton. Allowing, however, twenty-five shillings, and adding fifteen shillings per ton for royalty, though this will only be paid for a limited number of years, the sulphur would, at the outside, come to £2 a ton, and would thus have a clear profit of £4 per ton. A works decomposing 200 tons of salt-cake a-week would thus save £3200 a-year by the process. I hold in my hand a very elaborate statement of the plant in course of erection at Messrs. John Hutchinson and Co.'s works, and calculated for the treatment of the waste from the quantity of salt-cake just named. The total cost is calculated to amount to £1509, and adding 10 per cent. for contingencies, to £1660. Now, I do not believe that there is at this moment a manufacture in the

world which could show a similar extraordinary return for the outlay while producing an article of which 300,000 tons are consumed every year, and the demand for which is continually increasing. And I certainly think that the alkali manufacturers of our present time ought to be particularly well satisfied with the mercantile features of the process, without considering at all that it also enables them to remove an almost intolerable nuisance. You will, perhaps, object to my statement that I have not debited the sulphur at all for muriatic acid used in its manufacture, which is not inconsiderable, and is equal to about three tons of strong acid for every ton of sulphur. But I am of opinion that this acid, and particularly the weak acid, has no real value in this country, and is not likely to obtain it for many years, as there is still an enormous quantity run to waste into the rivers. Though this district utilizes the acid better than the Lancashire district, the quantity actually here run to waste, I mean either not employed at all or else unnecessarily employed in the manufacture of bleaching powder, bi-carbonate, &c., amounted according to a calculation based on the official returns of the Alkali Manufacturers' Association during 1866, to about 45 per cent. of the acid made, or to above 100,000 tons of strong acid which is more than double the quantity required for the recovery of the sulphur from all the waste produced in this district, which ought to yield about 15,000 tons of sulphur a year, representing a value of £90,000. Some will very likely be astonished at these figures, but if you will carefully examine the subject I am sure that you will find them correct. I shall be very happy, Mr. Chairman and Gentlemen, to give you any possible further information on those points which I failed to make sufficiently clear or about which you might wish to know any additional facts. I might have gone more fully into details in many parts of my subject, but I am very much afraid that I have already detained you a great deal too long.

The CHAIRMAN said, that whatever might be the opinion of the meeting as to the value of the paper—and that it was of great value no one could doubt—they were under a great obligation to Mr. Mond for the trouble he had taken in bringing so important a subject so very elaborately before them. He had no doubt that some of the

gentlemen present would be able to offer comments on the process: he, therefore, invited any comments on, or criticisms of, the process as detailed to them.

Mr. MOND, in reply to Mr. Archibald Stevenson, South Shields, said, that the waste treated in Holland during four years had been used for a great many purposes. The New Station at Utrecht had been built on waste, and a number of houses had been built upon it. A well used for domestic purposes, had been sunk through alkali waste, and the water, though very hard, employed for domestic purposes.

Mr. PATTINSON begged to move a vote of thanks to Mr. Mond for his very valuable paper. It appeared to him that Mr. Mond had solved the problem of disposing of soda waste. When the subject of utilising the waste products was mentioned to him he had always said that the principal objection was the expense, and it was believed that the substance recovered would not pay the expense of the recovery. Mr. Mond had discovered a process by which they could recover sulphur from waste at a cheap rate; and, therefore, chemical manufacturers ought to be obliged to him for his discovery.

Dr. LUNGE, in seconding the proposition, said that there were two other processes—Schaffner's and Hoffman's—both of which involved a very large amount of labour. They involved the removing of the waste from the vat, exposing it to the action of the air for several weeks. He was in Germany last year, and visited a manufactory where Schaffner's process had been introduced. He was told by the manager that they were modifying it, and bringing it nearer to Mr. Mond's. Schaffner's process had been given up wherever it had been worked, and Mr. Mond's introduced—at least as far as the blowing of air and the decomposition of the liquors were concerned. The only detail of Schaffner's process retained was the way in which the sulphur was converted into roll. As to Hoffman's process, Mr. Bell had mentioned at the last meeting that in his opinion Mr. Mond's was a decided improvement on it.

The CHAIRMAN said, that Mr. Mond's process had been alluded to in the report of the Commission appointed by Government with regard to the pollution of rivers. It might be that this process could be easily adopted; and so far as chemistry was concerned it was easy.

But they must remember that there was an increasing manufacture of bleaching powder and of bi-carbonate of soda, and that these manufactures took up the whole of the muriatic acid. The muriatic acid required in Mr. Mond's process would, therefore, be taken up in the manufacture of bleaching powder and bi-carbonate of soda. Thus, though the process was in itself valuable, its adoption for the purpose of reducing the nuisance felt from alkali waste was likely to be very limited, seeing that it would, to a considerable extent, interfere with the manufacture of bleaching powder.

Mr. MOND acknowledged the vote of thanks accorded to him for his paper, and in reply to the remarks of the Chairman, said that he would like to have an opportunity of going more fully than he would then be able to do into the question of utilising muriatic acid. As he had stated at the end of his paper, about 100,000 tons of strong muriatic acid flowed into the Tyne from alkali works during one year —or acid equal to 100,000 tons of strong muriatic acid. Believing that many manufacturers would be astonished at these figures, he explained how he had arrived at the conclusion. He found that so much salt was decomposed on the Tyne in the year, equal to so much strong acid. Now, the bleach and bi-carbonate made were also known, and allowing quantities, very liberally calculated, for their preparation he arrived at this remainder. The quantity of muriatic acid used for bleaching powder was considerably in excess of what was required. He believed that few manufacturers would be unable to find sufficient acid with which to treat their waste.

Mr. SCHOLFIELD thought that Mr. Mond's figures were erroneous as to the amount of salt decomposed.

Mr. MOND said, these were the figures given by Mr. Richard Cail as Secretary to the Alkali Association, who, if he had been present, might have given them better information on the question than he could supply.

Mr. SCHOLFIELD said, that if he had received information he would have provided himself with correct details on the subject.

The CHAIRMAN thought that the figures given by Mr. Cail were likely to be quite correct.

Mr. MOND said, that the quantity of acid required to do away with the waste nuisance, was from 20 to 25 per cent. of the acid.

From 1866 the production of bleaching powder had greatly increased; but the production of bi-carbonate had not increased; the amount produced was not at all equal to that in 1866, and it was, therefore, likely that there would be a good deal of muriatic acid to spare.

Mr. MOND, in reply to a question, said, that the amount of brimstone used in England was 50,000 tons a-year, and that the total amount exported from Sicily was 300,000 tons a-year. In reply to another question, he calculated that 40,000 tons of sulphur could be recovered from alkali waste in England in one year.

Newcastle-upon-Tyne Chemical Society.

ANNUAL MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE ON THURSDAY, OCTOBER 28TH, 1869.

I. LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

The SECRETARIES submitted to the meeting the Treasurer's accounts, as follows:—

Dr.

	£	s.	d.
To 74 Subscriptions	37	0	0
„ Balance	3	12	11
	£40	12	11
22 Subscriptions due	£11	0	0

Cr.

	£	s.	d.
By Advertising General and other Meetings	4	8	4
„ Subscriptions for "Annales des Mines" and "Zeitschrift für Analyt. Chemie"	3	5	0
„ Cost of Printing 1st Part of Transactions.—A. Reid ...	19	18	6
„ General Printing, Stationery, and Postage.—A. Reid...	9	4	3
„ Postage	3	3	4
„ Sundry Petty Expenses, including Commission for collecting Subscriptions	0	13	6
	£40	12	11
By Balance due Treasurer	£3	12	11

JOHN PATTINSON.

Compared with the Vouchers and found correct,

HENRY B. BRADY.

Newcastle-on-Tyne, Oct. 20th, 1869.

Mr. T. L. GREGSON, referring to the amount of unpaid subscriptions, remarked that it was very unsatisfactory, and said he thought that, as the subscriptions were very small, they ought to be paid in advance.

The PRESIDENT said, the Treasurer was going to remind those gentlemen who had overlooked that important matter, and he hoped that it would be remedied next year.

Mr. FREIRE MARRECO next read the

COMMITTEE'S REPORT.

In this the First Annual Meeting your Committee are of opinion that they may fairly congratulate the Society on the result of the first year's working.

At the close of last session the number of members was 96. Two gentlemen, who have left the neighbourhood, have since resigned their membership, and two new members stand for election to-night.

The ordinary meetings have been well attended, and the Committee trust that they have been on the whole successful in bringing before them subjects of practical interest.

The nucleus of a library of reference, which your Committee trust to increase in future years, has been formed.

It has been proposed that the Transactions of the Society should be issued in sheets, at intervals during the session, instead of as at present in one part at its termination. This change involving several serious considerations, your Committee are desirous that before they take any action the opinion of this meeting should be expressed on the point.

The financial state of the Society is satisfactory, although the delay on the part of some members in paying their subscriptions, has left a small balance at present due to the Treasurer.

Mr. T. L. GREGSON moved, and Mr. T. S. ALDER seconded the adoption of the Report.

Mr. FREIRE MARRECO said, that the amount expended on printing was rather formidable, but it extended over a period of fourteen months which was included in the accounts, as were also the preliminary expenses which had fallen upon the first year's subscriptions; but they hoped to reduce the amount expended in this way very materially, and to have more at their disposal for the other purposes of the Society.

The Report was adopted unanimously.

The PRESIDENT said, he was sorry to inform them that Mr. Brady

was confined to the house by indisposition, and on that account could not be present to introduce the motion of which he had given notice, for the following addition to Rule 7:—"Papers read before the Society shall not be published except in the Society's Transactions without the consent of the Committee." Perhaps they would consider that motion as submitted to their consideration. He should be very glad to hear any remarks they might have to make upon it.

Mr. B. S. PROCTOR said, suppose this were put as moved and seconded, even at the risk of the Committee appearing a house divided against itself, he would say that he thought it was a subject which had a practical and very important bearing upon the prospects and success of their Society, and was of opinion that it would be a resolution which would tell more than one way. He thought that if they limited the publication of papers to the Transactions of the Society, or to publication under the sanction and with the permission of the committee, they would throw certain obstacles in the way of members bringing papers, which would tell greatly to the disadvantage of the Society. He thought that one or two papers had already been read before the Society and published before their Transactions, and this might not have been the case if this rule had been in operation previously. Whatever stress they might put upon withholding the papers from the public until the publication of the Transactions, it would be very serious to tempt those who were able to prepare important papers to go elsewhere. He considered that the resolution would be a great disadvantage to the Society, and, therefore, his feeling was very strongly against the proposed change.

Mr. R. C. CLAPHAM concurred with the remarks of Mr. Proctor. They were met there that night as free and independent members to discuss these matters, and he thought it would be very desirable to take the question generally into consideration as to the publication of these Transactions. He was strongly of the impression that if these papers, some of which were very important, were published more frequently, it would add to the importance and value of the Society. As the question had been raised, he begged to suggest that the publication of the papers should take place more frequently. Whether it should be after each meeting would be a question for consideration; but at least more frequently than at present, so that the public might

have the advantage of many valuable papers that had been read. He thought also that it would be very useful to those who did not find it convenient to attend these meetings, if the papers were published and sent to their homes for them to read. He was of opinion that by adopting this course they would add materially to the strength and importance of the Society.

Mr. T. L. GREGSON thought the course suggested might lead to the publication of a number of papers not worth publishing, and who, he asked, was to pay for them? If the society had to pay for them, he thought the committee should judge as to what papers ought to be published, and that the society should have the benefit to be derived therefrom. If the papers were published after each meeting, instead of at the end of the year, the Transactions would not be necessary, because they had been forestalled.

The PRESIDENT said, to the best of his belief, in their rules there was one, which threw upon the committee the responsibility of deciding whether a paper should be read before the society or not, and having passed through that ordeal, every paper must be entered on the Transactions.

Mr. T. L. GREGSON remarked that the subject of a paper might be first-rate, but the treatment of it might be very miserable indeed.

The PRESIDENT said the ordinary way was, that a paper was submitted for approval, and then it was printed, as a matter of course. If, as had been recommended by Mr. Clapham, the papers were published more frequently, of course that would render unnecessary the publication by independent members. It seemed to him that if the committee could arrange for the publication of papers more frequently, that would meet the views which had been expressed.

Mr. B. S. PROCTOR said he would withdraw his opposition, if it could be made clear that the papers would be published monthly.

The PRESIDENT thought it would be better not to tie their hands.

Dr. LUNGE thought that, properly speaking, the motion could not be withdrawn without Mr. Brady's sanction.

The PRESIDENT observed, that properly it could not be brought forward. What they understood was, that the committee be recommended to endeavour to publish the papers more frequently.

Mr. R. C. CLAPHAM said, as it was now understood that the experi-

ment had to be tried, the expense would show itself at the end of the year.

Dr. LUNGE remarked that the conversation might have this effect, that members who had any papers to bring before the society would consent to suspend their publication.

The SECRETARY intimated that the committee would, after considering the question of expense, comply with the suggestion thrown out to publish the papers in sheets as early as they possibly could.

The PRESIDENT said, if they left the matter in his hands, he would endeavour during the next three months to work it in such a way as to be satisfactory to Mr. Proctor, Mr. Clapham, and other gentlemen.

The subject then dropped.

Mr. Horace Page was then unanimously elected a member.

The PRESIDENT then read his Address.

GENTLEMEN,—Upon the occasion of our assembling at the commencement of another session it becomes the duty of your President to offer a few remarks upon the amount of success which has attended our efforts as members of the Chemical Society of Newcastle during the past twelve months.

It was, perhaps, scarcely to be expected that any very great number of names would be enrolled as having joined our ranks during the first year of our existence, or that our Transactions should have been distinguished by a great number of papers. It has happened with us, as it does with most institutions like our own, that many persons, although approving of the object in view, hold back in order, in the first instance, to satisfy themselves of the character of our proceedings and of the probability of a fortunate issue to our labours.

Viewed in this not over sanguine light I do not think we have other than reason to congratulate ourselves on the position we at present occupy. We have upon our list of members the names of close on 100 gentlemen, and several subjects have engaged our attention at the evening meetings which have been listened to with interest and discussed with intelligence and zeal.

We had a useful and eminently practical paper from our worthy Treasurer on the relations between English and Foreign Alkalimetry

and Chlorimetry, in which, among other topics, he alluded to the fallacious number upon which our calculation in the first-named science is based, arising from our retaining the old atomic weight of soda instead of that determined by more careful analysis and estimation. There are, as Mr. Pattinson points out, good reasons for not hastily abandoning a system, erroneous though it be, which has grown into a trade usage, particularly when it is, as in the case in point, corrected by an error in the contrary direction. The inconvenience, however, of pursuing a plan opposed to truth, and opposed also to the practice of other nations with whom we may have dealings, is sufficiently obvious.

While upon the subject of discrepancies in ascertained results I perhaps may be permitted to allude to one of another character arising not from any numerical difference connected with the mode of estimate, but due, as I understand, to imperfect and already-abandoned methods of analyses being retained by operators charged with the research. Among other instances mentioned to me is a very important one, connected as it is with the determination of phosphorus in factitious manures, the real standard of which it is important should be known to the agriculturist.

It is scarcely necessary for me to point out how essential it is in the interests both of the buyer and seller that the simple truth should be expressed in any document professing to describe the composition of any commodity. I would urge, therefore, upon the attention of any member who may have reason to suspect the scientific accuracy of any process of assay to bring the subject in a suitable form for searching examination before the members of this Institute, in doing which he will confer a benefit on society and enlarge the sphere of our own usefulness.

Dr. Clemm favoured us with a paper on a new volumetric method of estimating the sulphuric acid in sulphates, and Mr. Henry Bowman with a translation of a work of Mons. Caron on the employment of magnesia as a heat-resisting material.

Were I asked to select from our proceedings the paper which best fulfils, according to my own views, the more immediate object contemplated by the founders of the Newcastle Chemical Society, I should probably give the preference to that written by Mr. B. S. Proctor on the Root of the Rhubarb Plant. Not its least recommendation is that it deals

with an old and, consequently, a deeply cherished fallacy which had led druggists and the medical profession, for the sake of mere appearance, to reject that portion of this valuable element of the pharmacopœia—the richest in the active principle of the drug—simply because it was less bright in colour and less beautiful to the eye than the other which paid for these attributes by a partial sacrifice of that which constitutes the whole value of the plant.

But the chief ground of preference, in the eyes of this Society, possessed by Mr. Proctor's paper, lies in the circumstance that it is an original work and not a mere compilation from the researches of others. In an excellent article in "Scientific Opinion" of this month, the editor regrets the absence of investigation of this nature from the generality of the printed transactions of provincial scientific societies. It must be a matter of congratulation to the lovers of science in the North of England, that from this sweeping condemnation an honourable exception is made of three societies, among which is to be found the Northumberland and Durham Naturalists' Field Club. To this might have been added the old volumes of the Natural History Society of Newcastle and the same counties. And why is this? Simply because we had our Winches, our Huttons, our Alders, and our Pattinsons; and we still have our Hancocks, our Attheys, our Bradys, our Normans and Howses, along with many others diligently, humbly, and with a devout heart studying nature, and contributing to the general spread of knowledge by giving us the result of their patient and never wearying labours. Now, why should we not do for chemistry, in which we profess to feel so deep an interest, that which our fellow-labourers, I may almost say fellow-citizens, have achieved in other branches of natural science? Are the objects of research more difficult of access than theirs? Have you necessarily to drag the bed of the ocean, filter its waters, or carefully scan every chip of rock obtained from our deepest mines, before you can find suitable scope for the exercise of those powers of original research which it is the especial province of this Society to promote and ought to be its continual aim to foster and develope? No such difficulty attends your endeavours to add a contribution to that store of knowledge comprehended in the boundless field presented by the study of chemistry. Objects of enquiry are trodden by the foot at every step. Many of

the rocks, minerals, and fossils of our carboniferous strata still remain unquestioned, and their composition unknown. Refuse products from our mines and our manufactures are carried off in waggon loads, in many cases without a thought being bestowed on the nature of their ingredients or the character of their composition; indeed I may venture to say from actual experience, that it is only when we come to apply the knowledge already acquired respecting the chemical nature of any particular substance that we stand in presence of the conviction that there still remains much to learn, and perhaps, sometimes, much to unlearn.

It will be perceived that the general tendency of the objects which were treated of in the communications read to the members, has been of a practical character, and although I am very far from discouraging the discussion of matters of abstract science, we must I imagine prepare ourselves for finding the pages of our Transactions will be generally occupied by subjects constituting the occupation of men engaged in practical pursuits. While, then, I would urge upon you the desirability of directing your attention, in your own interest and improvement, occasionally to questions of a purely scientific order, I cannot be unmindful of the value of a record filled for the greater part with the observations of practical chemists, made while in pursuit of their daily work. It seems to me that our Society will obtain for itself, by the publication of a volume thus compiled, a distinctive character which may, perhaps, render greater service to chemical science than any exclusive attempts on our parts to deal with questions of a purely abstract nature.

In this way I cannot look upon the paper prepared by Mr. R. C. Clapham, containing for the most part details of a purely statistical nature, otherwise than as an interesting and valuable contribution to our proceedings, embracing as it does, an able and comprehensive account of the manufacture of soda and its accompanying products; one of the, if not the most important of our local chemical processes. There is always a difficulty upon undertaking any enquiry in discovering the proper means of information, and where, it may be asked, would there be a more probable source of knowledge regarding the progress of the manufacture so ably treated by our colleague, Mr. Clapham, than in the Transactions of the Chemical Society of

Newcastle, a locality which has rendered itself famous in connection with the production of this most valuable alkali and other substances dependent upon its mode of manufacture? I would, therefore, respectfully urge upon gentlemen to favour us occasionally with some account of the past history, statistical as well as scientific, of those branches of chemical industry in which they may be personally engaged or interested. With such a compilation at hand we should not be left to lament, as Mr. Clapham does in his communication to us, that processes connected with the progress of the manufacture of soda are buried in oblivion, no trace of the method pursued being recorded, either to warn us of failure, or to devise changes suggested by the advance of knowledge which might convert former failures into our success.

A paper, of some length, and having great local interest, was prepared and read at a special meeting, last May, by Mr. Ludwig Mond, of Widnes, in Lancashire, on the recovery of sulphur from alkali waste. Although the plan described by Mr. Mond is by no means a perfect one, inasmuch as it only professes to recover a small portion of the sulphur used in our soda works, still it cannot be otherwise regarded than as an important contribution to our national industry, if, as Mr. Mond alleges, sulphur can be produced by its instrumentality at so low a price as about 25s. a ton. The process possesses the further recommendation, that the portion of the waste containing the remaining part of the sulphur is a much less offensive neighbour than is the blue alkali waste in its present form. Our colleague, Dr. Lunge, is occupied in feeling his way in this important extension of our northern manufactures, and Messrs. H. L. Pattinson are also engaged in the erection of the necessary plant, for what we must all hope will prove a satisfactory solution, so far as it goes, of this important question.

While touching upon the subject of applied chemistry in our own neighbourhood, I may also mention that the last-named firm are laying down the needful apparatus for the recovery of manganese by Mr. Weldon's mode of procedure, consisting of precipitating an oxide from the chloride of this metal (the latter a waste product obtained in the manufacture of bleaching powder), the oxide itself being subjected at the same time to considerable pressure.

At Walker, some experiments have been conducted, on a small scale so far, to determine the possibility of decomposing common salt, in a practical way, *i.e.* economically, by means of oxide of lead. The locality is not an inappropriate one, inasmuch as the same means, viz., an oxide of lead, as we all know, was used and abandoned by the ancestor and partner of some of the members of the present firm so long ago as 1806. I would not have it supposed that it follows that the same fate will necessarily fall to the lot of the present trials. The oxide is proposed to be used, I understand, in a hydrated condition, instead of an anhydrous one, such as litharge. A faithful statement of their predecessor's experience, such as I have proposed, should be preserved in the pages of our Transactions, could not fail to be most useful to those engaged in trying the, to some extent, previously trodden path pursued by those who traversed it above half a century ago.

The change which has taken place in the manufacture of sulphuric acid by the substitution of pyrites from Sicilian sulphur, has been the means of introducing a new and very important branch of manufacture upon the banks of the Tyne, viz., the smelting of copper. The ore most commonly used for this purpose is cupreous pyrites, a sulphide of iron containing more or less copper. It is obvious the limit at which such an ore can be profitably employed is much sooner reached where its value is to be estimated by being looked upon as a source of metal alone. The circumstance that there are large deposits of pyrites containing 2 or 3 per cent. of copper, and being besides rich in sulphur, has brought a large quantity of this mineral within the walls of our manufactories. The dry-way, necessitating the fusion of so large a mass of matter was attended with some loss of copper, and, consequently, the cementation process has been substituted with beneficial effects in the neighbourhood. In it the metallic iron for precipitating the copper, instead of being used in the form of wrought or pig iron, is applied in the spongy state, obviously a more suitable form, which is obtained by deoxidizing ferric oxide.

In alluding to the progress of chemical industry in our immediate vicinity, it would be wrong to overlook the rapid strides made in the employment of Esparto grass in the manufacture of paper, affecting, as it has done, the production of soda, by the aid of which, in remov-

ing the silica and resinous matter which abound in this vegetable substance, it alone has been able to take its present place in the arts. Six years have scarcely elapsed since Mr. Routledge first introduced this substitute for rags to the notice of the owners of our paper mills, and I perceive that last year close on 51,000 tons of this material were landed by the North-Eastern Railway, at Tyne Docks.

A step in the opposite direction may be recorded in connection with another branch of our chemical manufactures, one which was introduced on the banks of our river 150 years ago. The production of prussiate of potash, carried on by Mr. T. S. Bramwell and his ancestors at Heworth, for the last 80 years, has been abandoned by the firm of which this gentleman is the head. The extended use of aniline colours and foreign competition, he informs me, have led to the disappearance of this interesting process from the list of those pursued with success in our own neighbourhood.

Before dismissing the subject of the papers which have been read at our meetings, I would acknowledge the service which Dr. Lunge has rendered to anyone who may be anxious to refer to the Transactions of Foreign Scientific Institutions, by the compendious abstract he prepared of those papers likely to be interesting and useful to the Society.

Among the manifold applications of the truths revealed by means of chemical research, there is none more gratifying to the philosopher or to the philanthropist than that whereby chemistry is rendered subservient to the protection and promotion of the public health. It has been reserved almost for our own time to have it demonstrated that the observance of certain so-called sanitary regulations is connected by the closest bonds with the rate of mortality. This has been proved repeatedly in several large cities, at one time conspicuous for the high annual death-rate among their inhabitants, but which, by the authorities dealing with the causes of offence, now escape from the penalty which never fails to attend on the transgression of any great natural law. We need not, indeed, go far for an example in illustration of the doctrine I am enforcing, for in the very town in which we are now assembled, the rapid increase of population had outgrown as it were some of those means and appliances which must accompany the crowding together of a vast number of human beings on a small area of ground. The municipal authorities of Newcastle were no sooner

properly impressed with the gravity of their position, and convinced that the remedy and responsibility rested in their own hands, than the most vigorous measures were resorted to in order to grapple with the evil, and we have, in consequence, to congratulate ourselves on a remarkable alteration in the death-rate of this town. It may not be unworthy of mention that the first quarter of 1866 exhibited a mortality corresponding to 48·4 for every thousand of the inhabitants, and that the average for the whole of that year was a mere fraction within 40, viz., 39·7. Taking 10 years, ending with 1860, it was 35·4 per 1000. It cannot be otherwise than satisfactory to compare this with the three quarters of the present year, which is only 26·2, and for the last quarter the deaths only amounted to 23·3 per 1000; in short, from having held a most unenviable position among the most unhealthy towns of the empire, we are now conspicuous among those in which the mortality is the lowest.

Much of this, no doubt, has been accomplished without science having been able to detect the precise nature of those substances which exercise so fatal an influence on the health of the human race, but to the chemist it is perfectly intelligible that but a very slight change is required in the grouping of those elements constituting organic matter to convert that which is innocuous into what is most deadly. It is, perhaps, only since chemistry has made us acquainted with certain alkaloids that we have been made aware that in some cases the merest fraction of a grain of a substance, containing the self-same elements which constitute our daily food, suffices for the speedy, nay even the instantaneous, destruction of life. We are indebted, during the past year, to the labours of Dr. Matthiessen, of London, and my friend, Mr. C. R. A. Wright, now of Washington, for showing how the mere abstraction of the elements forming one equivalent of water changes morphia into what they have designated as apomorphia, in which the poisonous qualities of the former are so intensified that $\frac{1}{10}$ of a grain introduced under the skin was followed by the appearance of the most painful symptoms.

With these facts under our eyes, is it wonderful that infinitesimal doses of deleterious matter introduced daily into the system by means of the water we drink, or in a still more insidious form by means of the air we breathe, poisoning instead of purifying the blood at the very instant of its admission into the cells of the lungs, should exercise a

constantly depressing effect on the vitality of the individual, terminating with permanent loss of health and premature death.

Connected, then, as the science we are seeking to advance is with those objects which it is the especial province of the medical profession to watch over, I am sure I am only expressing the opinions of the members of our society in declaring that we should be very glad to be joined in our efforts in the extension of knowledge by a greater number of those gentlemen in whose very education an acquaintance with chemistry is considered indispensable.

It would scarcely be proper that in such an address as this aspires to be, our views should be restricted to that which concerns ourselves alone, and no allusion made to the prosecution of research by our leaders in chemical science.

There exist more reasons than one why Graham's experiments on the occlusion of gases by metals, claim a prominent place in a retrospect of the year's labours. In the first place, through them, confirmatory evidence has been afforded of the metallic nature of hydrogen; and in the second the great philosopher, whose investigations have led us up to this inference, has been removed by death in the midst of his intellectual work. By making a palladium wire the negative electrode of a small Bunsen's battery this lamented chemist succeeded in charging the metal with 936 times its volume of hydrogen by the decomposition of water. The wire had its length increased about $1\frac{1}{2}$ per cent. and its density reduced from 12·3 to 11·79. Singular enough the original wire thus lengthened, on having the occluded hydrogen expelled by heat, suffered in its longitudinal dimension a loss equal to its first addition. Thus a wire of 100 millimetres lengthened by the absorption of hydrogen to $101\frac{1}{2}$ millim^{s} contracted to $98\frac{1}{2}$ on the expulsion of the gas. The density of the metal, after the treatment, undergoing but little change, Graham conceived a certain mobility to be caused among the particles of palladium by the introduction of the hydrogen, thus producing the elongation, which disappeared on driving off the latter, and the same mobility continuing in operation the particles in a state of abnormal tension by the process of wire-drawing assume their natural relative position by righting themselves as the hydrogen escapes—thus the alteration in permanent length is compensated for by expansion in other directions.

There is much that is deeply interesting in thus adding this well-

known gaseous substance, or hydrogenium, as Graham called it, to our list of metals, for this group of elementary substances now contains, at ordinary temperatures, matter in the three forms under which we recognise it, and a metallic substance, instead of being as it was at the beginning of the century, a synonym for what was solid and ponderous, we now find in the form of the most attenuated and lightest of all known bodies. The alloy of palladium and hydrogenium is described as possessing marked properties, its conductivity is considerable, and it exhibits magnetic properties. From the density of the alloy, that of hydrogenium is calculated to be ·732, or about half that of magnesium.

In the hands of Sorby the mysterious lines afforded by the spectroscope have given evidence of the existence of a new element, called by him jargonium, which this chemist has since succeeded in obtaining in a separate form. The same instrument, employed by Norman Lockyer and others, affords strong reason for ascribing the flaming protuberances of the sun to the combustion of hydrogen, to be reckoned in quantities of millions of cubic miles.

Professor Roscoe has displaced the position hitherto occupied by the substance taken for vanadium among the elementary substances, by demonstrating that it really is a compound containing oxygen, and that vanadium itself is analogous to phosphorus.

Hitherto luminosity in flame has been ascribed as being due to the precipitation of solid matter on the surface of the burning gas, which solid matter, previous to its combustion, was for an instant raised to a point of brilliant incandescence. Frankland, guided by the fact that in some cases a great amount of light was emitted by flame where the product, although solid at ordinary temperatures, existed in the gaseous form at the time of its generation, was inclined to doubt the general conclusion arrived at by Sir Humphrey Davy on this subject. Frankland has confirmed the soundness of his own views by obtaining great luminosity by a mere increase of pressure. Dr. Abel has been engaged in some highly interesting researches on the power of explosive bodies, which, he shows, have their power greatly augmented by the manner in which their explosion is effected; and our fellow-townsman, Capt. Noble, of Elswick Ordnance Works, not only assisted in proving that the shape of gunpowder may play an important part in determining its strength, but he has placed in

the hands of those requiring it a most ingenious instrument, for measuring the velocity of projectiles, by means of which he obtained six readings marked by the passage of a ball along the length of a barrel of an ordinary rifle.

Tyndall has given some very remarkable instances of the chemical reactions produced by submitting the vapours of certain volatile substances, such as nitrite of amyl, hydrobromic, hydrochloric, and hydriodic acid, along with some others, to the action either of sun or the electric light.

A very important work from Mr. G. Gore, on hydrofluoric acid, has afforded us this remarkable substance in a liquid and anhydrous form.

In the more speculative branches of chemistry, Sir Benj. C. Brodie has contributed to the furtherance of science, a profound essay on the Calculus of Chemical operations, and Professor Williamson is continuing his investigations on the Atomic theory.

Referring, for a moment, to a topic alluded to in the inaugural address I had the honour of delivering to this body, I am glad to be able to state that the condition of our atmosphere, as affected by the burning of coal and the emission of objectionable vapours is now engaging the attention of a Committee, with the Mayor at its head, appointed to enquire into the subject. I trust, now that the public mind has been directed to the evil of a smoke-obscured sky, or poisoned air, before long, the inhabitants will experience a happy change from the result of the labours of those charged with the investigation. It is only, however, due to our chemical manufacturers to state that they are fully alive to the importance of not permitting any unnecessary escape of vapours, having an actual value to themselves, and very inconvenient to others when set at large, and therefore, that they do not intend to rest content with the occasional visit of the government inspector, or of their own superintendents, but are making arrangements for the permanent and continuous sampling of the gases after they have passed their condensing apparatus. Their observations in this direction, will, I feel assured, be much lightened by a very ingenious aspirator, constructed by one of our members, Mr. Swan, now on the table, to which I would invite inspection. I have, myself, been engaged for some time in an examination of the state of combustion experienced by the fuel in our blast-furnaces, and I am

so satisfied that a proper study of the phenomena attending it involves considerations of the utmost importance to the iron-smelter, that I intend availing myself largely of the facilities which the apparatus of Mr. Swan is capable of affording.

I must now bring to a close those observations which the occasion of the beginning of another season of work has suggested to my mind, and I do so with an earnest appeal to each member of the Chemical Society that he will devote a portion of that spare time he has at command, or a portion of that leisure every one can create for himself, in adding one more observation, however simple, to that catalogue of facts upon which is founded one of the most sublime and glorious sciences developed by the intellect of man.

Mr. GREGSON proposed a vote of thanks to Mr. Ald. Bell for his able address, and remarked in the words of Goldsmith,

> "And still the wonder grew,
> How one small head could carry all he knew."

Mr. P. A. BERKLY had great pleasure in seconding the motion.

The PRESIDENT said, he was greatly indebted to Mr. Gregson for speaking so of his efforts, and he could only say, that he should be truly happy if, at the end of the year, he found his observatiens had been acted upon. He should accept as one of the most conclusive proofs of Mr. Gregson's admiration and appreciation of what had been advanced, if that gentleman would accept the invitation in the concluding paragraph of the address, and favour them with a paper of his own on those subjects on which he was so well calculated to instruct them. He concluded by intimating that the officers of last year were all re-elected.

Mr. CLAPHAM moved a vote of thanks to the Secretaries for the able manner in which they had discharged their duties.

Mr. G. T. FRANCE seconded the motion.

The PRESIDENT, in putting it to the meeting said, he was sure that Mr. Marreco deserved their thanks, and his (the President's) especially, if it were only for the number of letters he wrote to him about the affairs of the Society.

The motion was carried by acclamation.

The officers of last year were re-elected by a large majority and the meeting then separated.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, NOVEMBER 25TH, 1869.

I. LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Mr. C. R. A. Wright was elected a member of the Society.

The SECRETARY read, for the authors, the following paper,

ON THE ESTIMATION OF PEROXIDE OF MANGANESE IN MANGANESE ORES.

BY E. SHERER AND G. RUMPF.

THE method of Fresenius and Will for the estimation of manganese, to which for so long preference has been given, has recently been objected to in England by the consumers of the ore. Attention has been called to the fact that this method shows indeed correctly the proportion of binoxide of manganese in the sample, but on the other hand does not represent its value as an agent for the production of chlorine. The reason for this is that metallic iron, or its proto-compounds, which are often present in the ore, absorb oxygen to form a sesquisalt when the sample is treated with hydrochloric acid, but remain unchanged when treated with concentrated sulphuric acid and oxalate of soda. In consequence of this the method of Fresenius and Will is still used in some laboratories, while in others it has been superseded by the iron method, from which circumstance arises an uncertainty in the valuation of the article, disagreeable alike to pur-

chasers and sellers. This has led us to subject the various methods to a careful examination and comparison one with another. Our object was, first, to ascertain by a series of experiments how far the estimations by the same method agreed one with the other; and secondly, to discover the causes of the differences that have been observed between the results obtained by the different methods.

A sample of American manganese was used for our experiments, a large quantity was pounded, well mixed, and carefully dried at 100° centigrade.

The moisture amounted to 0·749 per cent. Quantities of the dry manganese were then at once weighed off for the various tests.

I.—ACCORDING TO THE METHOD OF FRESENIUS AND WILL.

Years of experience having proved to us the constancy of results obtained by this method, we considered that three trials would be sufficient. These served to show the actual amount of binoxide in the sample :—

Manganese used.	Carbonic acid evolved.	Peroxide per cent.
2·1098 grms.	1·355 grms.	63·49
1·5103 grms.	0·970 grms.	63·49
1·3645 grms.	0·8770 grms.	63·53

II.—ACCORDING TO THE IRON METHOD.

The estimations by this process were conducted in the following manner: a weighed quantity of pianoforte wire was dissolved in hydrochloric acid in a glass flask; carbonic acid was conducted into the flask during the operation to prevent the oxydation of the proto-chloride of iron produced by oxygen from the atmosphere; when complete solution had taken place a small tube containing an accurately weighed quantity of the manganese was dropped into the flask; the mixture boiled until complete decomposition had taken place, and then cooled in the stream of carbonic acid. The excess of the proto-chloride was then estimated in the usual way by means of a standard solution of bichromate of potash. The weight of the iron wire multiplied by 0·997 was taken as the amount of pure iron used.

Manganese.	Pure iron.	Bichrome.	Peroxide per cent.
0·8748 grms.	1·0796 grms.	22·7cc	62·00
0·7504 grms.	1·03668 grms.	25·85cc	62·35
0·8746 grms.	1·1686 grms.	27·65cc	62·54
0·7604 grms.	0·9692 grms.	21·45cc	62·20
0·9312 grms.	1·3846 grms.	38·2cc	61·97
0·5467 grms.	1·2183 grms.	46·8cc	61 39
1·1356 grms.	1·0787 grms.	10·2cc	62·07
1·0446 grms.	1·0792 grms.	14·6cc	62·01

We, therefore, conclude from these figures that this method, proceeded with in the way we have described, does not yield accurate results.

III.—THE ESTIMATION OF MANGANESE BY THE FORMATION OF CHLORIDE OF LIME.

The advantages that this method would seem to have over all others is that the manganese is treated in precisely the same manner in the laboratory as it is on the large scale in the manufacture of bleaching powder.

Weighed quantities of the samples were treated with strong hydrochloric acid in a small flask until complete decomposition had taken place; the chlorine produced was conducted first into a second flask, and then into a U tube; both of which were filled with weak milk of lime. The resulting chloride of lime was then tested by Gay Lussac's method with arsenious acid.

Manganese.	Arsenious acid.	Peroxide per cent.
1·0561 grms.	53·9cc	62·60
1·1845 grms.	56·05cc	63·39
1·2045 grms.	61·7cc	62·83
0·4222 grms.	20·7cc	60·14
0·8102 grms.	39·75cc	60·18
0·4929 grms.	23·15cc	59·06
0·4875 grms.	23·15cc	58·15

These figures sufficiently prove that no accurate results are to be obtained by this method.

We next experimented on Bunsen's method. Weighed quantities of manganese were dissolved in hydrochloric acid in the same manner as in the preceding. The escaping chlorine was received in a strong solution of iodide of potassium, and the liberated iodine subsequently estimated by means of a standard solution of hyposulphite of soda and a solution of starch. To prevent the solution of iodide of potassium from being sucked back into the generating flask a few small pieces of magnesite were introduced with the manganese, so that a continual slight evolution of carbonic acid took place through the solution.

The solution of hyposulphite of soda was standardized by means of carefully-prepared pure iodine.

3·0436 grammes of iodine were dissolved in iodide of potassium and diluted to 250 cubic centimetres; 10 cubic centimetres of this iodine solution required 17·90 cubic centimetres of the hyposulphite of soda solution; 1000 cubic centimetres of hyposulphite of soda solution would, therefore, correspond to 2·3294 grammes of binoxide of manganese. A second trial was made, in which 2·4767 grammes of pure iodine were used: 1000 cubic centimetres of the hyposulphite were found to correspond to 2·3319 grammes of binoxide of manganese, which two results closely agree.

As it did not seem advisable to use less than half a gramme of manganese to each test, the resulting solution of iodine in iodide of potassium was diluted to 250 cubic centimetres, and 50 cubic centimetres used for each trial.

Manganese.	Hyposulphite.	Peroxide per cent.
0·6472 grms.	35·8cc	62·76
0·6427 grms.	34·6cc	62·73
0·6035 grms.	32·5cc	62·73

These results agreed so closely that we considered further estimations by this method to be quite unnecessary.

If we now compare the various results we have:—

	Average.	Greatest Dif.
1.—According to Fresenius and Will's method	63·50 %	0·04
2.— „ „ the Iron method	62·06 %	1·15 %
3.— „ „ „ Chloride of Lime estimation	60·90 %	5·24 %
4.— „ „ „ Bunsen's method	62·74 %	0·03 %

From this we see that only the methods of Fresenius and Will and that of Bunsen give constant results, and of these two the latter must undoubtedly represent the value of the ore as an agent for the production of chlorine, since only the chlorine evolved from the sample in the form of gas is estimated; we, therefore, may conclude that the method of Fresenius and Will gives results too high, while the iron and chloride of lime methods give results much too low. Our next step was to discover the reason for these differences. The cause of the high results obtained by the method of Fresenius and Will has already been mentioned at the commencement of this paper. We did not consider it necessary to examine whether or no the sample on which we were operating actually contained iron or its proto-compounds. The object we had in view was purely a practical one, and the possibility of the presence of such iron compounds in the ore cannot be disputed. The following experiment proves that a very considerable addition of metallic iron to the sample will not in the slightest degree affect the accuracy of the results obtained by this method. 1·4706 grammes of metallic iron were added to 2·4745 grammes of the manganese, concentrated sulphuric acid added, and the test proceeded with as usual, the result obtained, namely, 63·13 per cent. differed only by 0·37 per cent. from the average of the three previous trials made by this method, notwithstanding that the amount of iron added was sufficient to render three-quarters of the manganese unavailable as an oxydizing agent when dissolved in hydrochloric acid. Since varieties of manganese do frequently contain the iron compounds of which we have spoken, we did not further investigate the question whether or no this was the only cause of the too high results which this method yields under certain circumstances. The reason of the uncertainty of the results obtained by the iron method, as also of their being generally too low, we discovered to be, that a slight loss of chlorine generally takes place during the operation. When the manganese is added to the protochloride solution, a greater or less quantity frequently floats for a short time on the surface of the liquid; a small quantity of chlorine is evolved from this which has no opportunity of acting on the iron solution, and is carried off with the stream of carbonic acid. We convinced ourselves that this was the case by conducting the escaping gas into a second flask which contained a small

quantity of weak protochloride of iron solution. As soon as the manganese rose to the surface of the liquid in the decomposing flask, a yellow colour appeared in the liquid in the second flask, proving that a portion of the chlorine from the manganese escaped into the second flask, and there formed a sesqui-chloride of iron. The contents of the two flasks were then separately tested with the chromate of potash solution. From the first flask alone the sample would seem to contain 2 per cent. less than it actually does contain.

The quantity of manganese weighed off...	0·8129 grms.	
Pure iron in first flask	1·06519 grms.	
Chromate of potash used for first flask...	25·8cc	
		= 60·48 %
Pure iron in second flask	0·989 grms.	
Chromate of potash used for second flask	57·02cc	
		= 1·99 %
		62·47 % peroxide.

This figure agrees tolerably well with the result obtained by Bunsen's method. This method can, therefore, be made to yield accurate results, but only by a difficult and troublesome arrangement, which makes it more complicated and longer than that of Bunsen. Instead of metallic iron, a protosalt, such as the double sulphate of iron and ammonia, is sometimes used in the iron process and the decomposition of the manganese effected by means of weak sulphuric acid. No loss of gas probably takes place in this method. Experience has shown that it is very difficult to obtain such salts perfectly free both from sesqui-compounds of iron and moisture; we did not, therefore, experiment on this method.

The reason of the great irregularity in the results obtained by the chloride of lime method is easily perceived. At 40° Centigrade chloride of lime commences to be decomposed with formation of chlorate of lime: so that a greater or less speed in the evolution of the gas will produce a greater or less warmth in the milk of lime, and, consequently, more or less decompose it. We endeavoured, by external cooling, to prevent this warming and decomposition but with no satisfactory results, as the following figures will show. The milk of lime was cooled by the external application of a mixture of sulphate of soda and hydrochloric acid.

Manganese.	Arsenious acid.	Peroxide per cent.
0·3633 grms.	18·9cc	63·80
0·7778 grms.	38·45cc	60·64
0·8185 grms.	40·5cc	60·70
0·9204 grms.	46·25cc	61·64
0·6975 grms.	35·69cc	62·69
0·5581 grms.	26·85cc	59·01

This method can, therefore, only be used for approximate results. In estimating the manganese by Bunsen's method, the iodine liberated by the chlorine should be tested as soon as possible after the decomposition, since the solution, in presence of air, gradually reacts on the iodide of potassium and liberates iodine. The iodine solutions from the tests numbered 1 and 2 were again tested after standing twenty-four hours, and the results were both over 65 per cent.

In conclusion, therefore, we may decide that the results obtained by the method of Bunsen exceed all others in accuracy. Its simplicity also is greater than that of the iron method, as it requires only one weighing, and does not need a stream of carbonic acid.

At the mines where tests continually have to be made, the method of Fresenius and Will would certainly appear to be the best and simplest, so much the more that here no accidental admixture of iron can have taken place.

In conclusion, we would suggest that it is much more reasonable to represent the value of the manganese rather by chlorimetrical degrees than by the per centage of binoxide. Neither the method of Bunsen nor the iron method represents the per centage of binoxide, but the relative value of the ore as an agent for the production of chlorine, and, therefore, a similar form of expression for the value of manganese and chloride of lime seems to us desirable.

The PRESIDENT—The subject is one of great practical interest, and we shall be glad to hear the observations of members upon it.

Mr. JOHN PATTINSON—With regard to the last suggestion on the paper, I think that instead of using chlorimetrical degrees, it would

answer all useful purposes to employ the term "available peroxide." Chlorimetrical degrees are not used in England. With respect to other methods, the experiments cited by the authors are extremely interesting. I have no doubt the iron method which they describe, performed as they give it, would give erroneous results. I have long known that chlorine is occasionally evolved when hydrochloric acid is employed in the iron process. It may be detected by smell, and also by ozone paper. I am quite sure, however, that the iron process is capable of giving accurate results. I follow this process in my own laboratory, and I obtain results which are generally identical, and seldom, or never, vary by more than ·1 per cent. peroxide. It is much simpler and easier in manipulation than the Bunsen method. At present, I shall say no more, as I hope to present a paper on this process to the Society.

Mr. WRIGHT—While agreeing with some of Mr. Sherer's results, and his deductions therefrom, I must confess that my own experience does not altogether confirm his statements. His analytical numbers show that with Bunsen's method, very closely agreeing results are obtainable, whilst Fresenius' method gives results also agreeing among themselves, but indicating a higher per centage of "available binoxide" than that deduced from Bunsen's process. I have always found a very close agreement between the results obtainable by each method, provided the manganese ore were perfectly free from carbonates; careful manipulation, being of course, indispensable. As Mr. Sherer makes no mention of having satisfied himself of the absence of carbonates in the sample he operated on, possibly the higher result he obtained by Fresenius' method may be explained by the presence of a small quantity of carbonate of calcium; a not unfrequent circumstance in some descriptions of manganese ores. In his determinations, by the use of iron wire, Mr. Sherer has shown that it is difficult to avoid loss of chlorine if hydrochloric acid is used. In this respect my own experience bears out his conclusion, but I have always got results agreeing among themselves and with those got by the two forementioned methods, if pure sulphuric acid is used; and, in connection with this, I may remark, that the ammonium ferrous sulphate ($FeSO_4 + (NH_4)_2 SO_4 + 6H_2O$) is a much more convenient substance to operate with than iron wire, either for man-

ganese estimations or for standardizing permanganate solutions for iron assays, as it contains exactly 1-7th of its weight of iron, and can be readily obtained in a pure state by adding alcohol to a hot concentrated aqueous solution of the salt, washing the precipitated crystals with alcohol, and drying on filter paper; as thus obtained, the salt has little tendency to efflorescence or peroxidation, and may be kept unchanged for years in an ordinary corked phial. The irregular results obtained by Mr. Sherer, with the bleaching powder process, would certainly indicate that this method is valueless; he, however, mentions Gay Lussac's method of estimation by arsenious oxide. I have never been able to obtain accordant results by this mode of operation (*i.e.*, use of an *acid* solution of arsenious oxide), but have always obtained uniform results by Penot's process (use of *alkaline* arsenite in excess, the unoxydized arsenite being determined by standard iodine solution); by this latter method, however, the chlorine indicated in the bleaching powder is simply that due to hypo-chlorite; chlorates being ineffective in peroxidizing an alkaline arsenite solution. Mohr's process for manganese ores (distillation with hydrochloric acid, reception of evolved chlorine in alkaline arsenite solution of known strength, and determination of excess of arsenite by iodine solution) has yielded me values absolutely agreeing with those furnished by Bunsen's, Fresenius', and Otto's (iron) methods. Since, however, an alkaline arsenite solution soon alters in strength by keeping (from peroxidation), it is more convenient to keep a standard solution of arsenious oxide in hydrochloric acid, adding an excess of bicarbonate of soda to a measured quantity, when a known alkaline solution is required. A correspondent writes to the "Chemical News," Oct. 11th, 1867, stating that he employs a modification of Mohr's method, consisting of the absorption of liberated chlorine by sodium carbonate, and estimation of the hypo-chlorite so produced by alkaline arsenite solution of known strength. In a paper published in the "Chemical News," for Oct. 4th, 1867, I described the results of experiments on the same sample of manganese ore by the process of Fresenius, Bunsen, Otto, and Mohr, and likewise by Price's method (boiling with hydrochloric acid and a known weight of arsenious oxide, in a flask with a bulb tube attached to prevent loss by volatilization of arsenious chloride, and estimation of

unaltered arsenious oxide by permanganate); also of a slight modification of Price's method, viz., use of sulphuric instead of hydrochloric acid, whereby more uniform results are obtained without fear of loss of arsenic by volatilization. All these processes yielded closely agreeing results, none differing so much as 0·2 per cent. from the mean. Both Price's process and its modification, however, are subject to a slight error from the fact that the amount of permanganate required to oxidize a known quantity of arsenious oxide is liable to vary with the state of dilution of the liquid, the amount of free acid, &c., &c.; these variations, however, are only of negligible amount, if the permanganate solution be standardized by a solution of arsenious oxide in sulphuric acid to commence with, and if the excess of arsenious oxide to be determined be not too large. In the same paper I described experiments whereby it was shown that when known quantities of ammonium ferrous sulphate were added to manganese ore in the estimation by Fresenius' method, the carbonic acid evolved did *not* correspond to the "available binoxide" present, but only to that left after peroxidation of the iron. In conclusion, I may say that I believe the true cause of the higher results frequently obtained by Fresenius' method to be careless manipulation, such as, for example, weighing the apparatus while still hot, &c., &c.; the fact that it is essential that the amount of carbonate in the sample should be estimated, or its absence proved, is certainly an argument in favour of methods which only require a single determination to be made. The chief objection to Bunsen's method appears to be the smallness of the quantity operated on, and the increased difficulty of manipulation: in addition, Mr. Sherer finds that erroneous results are obtained by allowing the liberated iodine to remain some hours before titration; but how the air can act on this iodine solution to such an extent, while it is without effect on the solution of iodine in potassium iodide used as standard iodine solution, I am at a loss to understand.

Mr. Freire-Marreco—When I was interested in manganese assays, I used to heat the sample with a known quantity (in excess) of potassium oxalate and sulphuric acid, and determine the undestroyed oxalate with permanganate solution. The process gives sharp results and you are not encumbered with questions of possible carbonates.

Mr. C. R. A. WRIGHT—The method mentioned by Mr. Marreco has yielded me results closely agreeing with those deduced from the evolved carbonic acid; pure oxalic acid may be advantageously used in this process, but if the solution is dilute, the action of the manganese is greatly retarded; it is advisable to standardize the permanganate by an oxalic acid solution of known strength, and not by the iron processes.

Mr. JOHN PATTINSON—I would suggest that the discrepancies as to the effect of ferrous compounds may be explained, by Mr. Wright having used a soluble iron salt instead of metallic iron, which is more slowly acted on.

Mr. C. R. A. WRIGHT—Although it is not referred to in my paper, I have also observed that the addition of pulverised magnetic oxide of iron (smithy scales) diminished the amount of evolved carbonic acid, though not to an extent absolutely corresponding with the ferrous compound employed; in the case of a manganese ore containing ferrous compounds, however, the particles of ferrous substance, being in close proximity to the manganese particles, would be more likely to be peroxidised than they would be in the case of a pure mixture of two substances in small fragments or powder, as in Mr. Sherer's experiments with iron filings, or mine with smithy scales. I think the differences observed may be accounted for by a small per centage of calcium carbonate.

Mr. JOHN PATTINSON—I think they would hardly make such a mistake, as carbonates are always carefully sought for by any one accustomed to manganese assays.

The PRESIDENT—Manganese ores are very often found in a lime-stone formation; it is only natural then that they should sometimes contain carbonate of lime.

Mr. JOHN PATTINSON—I happen to have tested almost every sample of Virginian ore brought to England, and not one of them contained carbonates.

Mr. G. T. FRANCE (in reply to the President) said, we do not test our manganese samples very carefully ourselves, we send them to Mr. Pattinson or Teschemacher and Smith. We get such varying results that we never buy on our own testing, and the merchants will only sell on these two tests.

Mr. AYNSLEY COOKE (in answer to the President) said, we have not done much yet in the way of assaying manganese; we are trying to do so.

Mr. G. T. FRANCE—A good practical test of its power of evolving chlorine would be very satisfactory.

Mr. C. R. A. WRIGHT—In any test depending on the formation of chloride of lime, the results would depend on several conditions. Other things being equal, if you take two limes, one fat the other poor, in the chambers you will get very different results.

The PRESIDENT—When I was practically concerned in the manufacture of bleach, we always considered it important to avoid forcing the process so as to raise the temperature.

Mr. G. T. FRANCE—I should like some explanation of this fact. I had two different ores both testing as nearly as possible 70 per cent., I tried known quantities of both in the stills and found a great difference in the chlorine they evolved.

Mr. C. R. A. WRIGHT—Some samples of manganese are so hard as to be practically unattackable.

Mr. JOHN PATTINSON—In acting on some manganese ores with hydrochloric acid you get a semi-gelatinous film of silica deposited on the outside of the lump which effectually checks the action.

Mr. G. T. FRANCE was understood to say that both samples were thoroughly attacked.

On the motion of the PRESIDENT, seconded by Mr. PATTINSON, the thanks of the Society were given to the authors of the paper.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, DECEMBER 23RD, 1869.

I. LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

The following names were read for the first time:—Mr. Wm. Blench, Gateshead, and Mr. Thos. Brayshay, Stockton.

The PRESIDENT read—

SOME ACCOUNT OF THE MANUFACTURE OF FACTITIOUS PYRITES AND ITS APPLICATION TO THE PRODUCTION OF SULPHURIC ACID.

ONE of the objects of our Society is to provide a convenient means of recording any modification or improvement introduced in those numerous chemical processes carried on in our neighbourhood, with one or other of which the greater number of our members are more or less directly connected.

It is needless for me to insist upon the interest, as well as the value, of a history which should present for our instruction and guidance a correct account of the various steps which have led up to the development and improvement of any branch of industrial art.

There is, however, another class of discoveries which incurs a greater risk of being overlooked and forgotten than those actually put in practice in our manufactories, I mean the unsuccessful attempts which have been made at ameliorating any operation required in the application of manufacturing science.

Thoughtful men have frequently dwelt on the desirability of our possessing a faithful account of unsuccessful experiments, which, though they may have failed in answering the expectations of their promoters, probably are never altogether wanting in some features of sufficient importance to justify our not dismissing them as utterly unworthy of notice.

In the absence of any subject for this evening's meeting I have, somewhat hurriedly, drawn up a short sketch of an attempt of my own to recover sulphur from soda-maker's waste. I enter upon this the more willingly because the question is a large and important one, and although the process was abandoned at the time, the trials at Washington nevertheless afforded some results, the knowledge of which may hereafter prove useful, and cannot, I trust, under any circumstances, be devoid of interest to the members of the Newcastle Chemical Society.

At the period in question (1853) the high price of sulphur had led the soda-makers in this district to adopt the use of pyrites in the manufacture of sulphuric acid, and as this mineral was not then such an abundant article of commerce as an extended application has since rendered it, its value in the market was soon affected by the sudden expansion of the demand made upon the mines affording it.

I need scarcely remind you that, practically speaking, nearly the whole of the sulphur intercepted in the vitriol chambers finds its way, in the process of making soda, into the tank waste, nor need I dwell on the soda-maker's regrets as he watches, day by day, that offensive substance carrying out of his works the most costly article which enters within its walls.

Previous attempts to recover the sulphur thus lost were, I think, all combined directly or indirectly with what in former years was also exclusively a waste product, viz., hydrochloric acid.

Soluble compounds of sulphur and calcium were allowed to be generated by exposure of the waste to the atmosphere, from which the sulphur was precipitated by means of the acid just named. This idea has been recently revived by Mr. Mond, whose process promises a greater amount of success than those which preceded it, inasmuch as it is alleged he obtains a larger produce of soluble sulphide by a more speedy oxidation of the tank waste, which is induced by an artificial current of air being passed through it.

Another plan, formerly tried, consisted in converting the sulphur into sulphuretted hydrogen, and burning the gas so as therefrom to generate sulphurous acid, which was treated in the ordinary way. Sulphuretted hydrogen was also obtained by passing carbonic acid over the tank waste, the carbonic acid being got by adding hydrochloric acid to chalk.

The large quantity of lime, besides that united with sulphur, which exists in soda-maker's waste, and the difficulty of burning sulphuretted hydrogen so as to produce sulphurous acid and water, without at the same time precipitating sulphur, caused these latter plans to be abandoned, even when sulphur was as high as £12 per ton, to which price it rose under the absurd monopoly granted by the Sicilian Government to a French Company.

There is, so far as I know, no plan hitherto proposed which can accomplish that which Mr. Mond professes can be effected by his process, viz., the production of sulphur at 25s. 5d. per ton. Admitting his power to do this, the defect, it appears to me, of this mode of dealing with the subject is that he only calculates upon recovering half the sulphur, and this by the application of about a fourth of all the hydrochloric acid obtained by decomposing the salt employed in the operation of making soda. Included in the price of 25s. 5d. there is nothing allowed for any value possessed by the hydrochloric acid, and although this, to the extent of one fourth of that generated in a soda work, may be true enough at the present day, it seems scarcely safe to assume that so powerful a reagent as this acid undoubtedly is can be regarded as worthless for all time. A change of conditions which should confer a value of 30s. a ton on liquid hydrochloric acid would at once add £4 10s. to the 25s. 5d., seeing that Mr. Mond admits that three tons of strong acid are absorbed for each ton of sulphur regenerated.

The introduction of pyrites into the soda works as a source of sulphur, placed another waste product at the disposal of the manufacturer, and my association with the smelting of iron led me to consider whether, in a blast furnace, such as is used in that operation, the tank waste would not yield up its sulphur to the iron of the burnt pyrites, which latter under such treatment as that proposed would of course be deoxidized. If so, then the addition of silica and alumina, so as

to obtain a perfectly fusible slag, would free the resulting sulphide of iron from the lime existing in the tank waste.

After the necessary preliminary experiments, made in an ordinary cupola, to satisfy myself of the general correctness of these expectations, a small blast furnace was built about 25 feet high and 10 feet across the widest part. The air, heated to about 650° F., was supplied by a pair of small blowing cylinders, 24 in. diam., with a stroke of 4 feet, and capable of supplying about 1200 to 1500 cubic ft. of air per minute.

Into this furnace coke was introduced along with the soda waste and burnt pyrites, in the condition in which they came from the works. To the charge was added a portion of common clay to act as the flux. In appearance the process resembled an iron furnace in operation—the materials were charged as they are in smelting that metal, while a constant flow of slag was maintained below, and the sulphide, or factitious pyrites as we designated it, was tapped out every morning and evening.

To some extent the ashes from the reverberatory furnaces were employed instead of the clay. These consisted of the earthy portions of the coal which served as a flux, while the unconsumed carbon they contained served instead of so much coke. Thus three waste products, vat refuse, burnt pyrites, and furnace ashes, were got rid of, and a hard slag suitable for road-making produced in their stead.

So far as the mere union of the sulphur and iron was concerned, the process was entirely successful, but, probably as in all novel operations, we had some serious obstacles to contend with. The introduction of so large a quantity of a moist material like soda waste greatly affected the free passage of the blast through the furnace. As a simple physical difficulty this might have been avoided by some previous preparation of the soda waste itself, but unfortunately the value of the material for which our factitious pyrites had to be a substitute, was not such as to permit any great addition to the cost of manufacture. Our greatest trouble, however, was caused by the destruction of the sides of the lower parts of the furnace itself by the corroding action of the fused sulphide of iron. This led to occasional stoppages and somewhat costly repairs, which amounted at last to more than the operation, as a financial one, could support. The presence of a certain amount of oxide of iron which always accompanied

the sulphide, probably was the cause of the brick work being thus destroyed; but notwithstanding the known affinity which oxides of iron have for the substances which constitute our fire clays, it is not impossible that by a proper use of means of cooling, in the way frequently had recourse to in our iron furnaces, this difficulty might have been modified or overcome.

Notwithstanding the impediments just described, as much as 70 tons of factitious pyrites per week was frequently obtained from the furnace employed at Washington. Something like 15 cwts. of coke per ton were required for its production, and when everything went well it was obtained at as low a cost as 12s. 6d. to 15s. Taking into account the content of sulphur, this amounted to something under 6d. per cent. per ton, and as the price of natural pyrites at the time was about 1s. per cent., there existed grounds for hoping the process might have been a useful addition to an important section of our local manufactures. But our calculations as to cost of manufacture and commercial success were also interfered with by a considerable change in the market value of small coal, which rose to about double its former price. And not only were the expenses of production thus materially enhanced, but at the same time the discovery of new mines of pyrites on the Continent was attended with a considerable diminution in the value of the native sulphide against which we had to compete.

I will now briefly describe to you the different products resulting from the mode of treatment just described.

Volatile Matters.—Although the waste gases from iron smelting were, at that time, frequently made use of for raising steam, &c., we were unable to apply the carbonic oxide, which left the factitious pyrites furnace, for this or any other purpose, because it was frequently loaded with watery vapour to such an extent as to be absolutely incombustible. We possess no analysis of the gases, and, therefore, I am unable to state their constitution as they issued from the throat of the furnace. Judging by the smell, the presence of sulphurous acid was of rare occurrence, and when sulphur was recognised as escaping, it was in the form of sulphuretted hydrogen, but the odour of this well-known substance was by no means a constant accompaniment of the operation. That sulphur, in some shape

or another, was given off there was little doubt; firstly, because there was a certain deficiency which I was unable to account in any other way; and, secondly, although sulphur was not detected either in the condition of sulphurous acid or sulphuretted hydrogen, the offensively smelling vapours which were emitted, left me no alternative but of ascribing them to the presence of sulphur, in some form or other.

Slag.—In the smelting of iron, whenever the time for which the ore is exposed to the reducing gases of the furnace is such and the temperature is suitable, the whole or nearly the whole of the oxide of iron is deoxidized, so that little or no metal finds its way into the slag. If, on the contrary, a heavier load of ore is added to a given weight of coke, which is done when white iron is sought for, a portion of oxide escapes reduction and passes off in the slag. The same conditions prevailed in the manufacture of factitious pyrites, for occasionally the slag was of a fine light blue colour, indicative of a small quantity only of oxide of iron. There was, however, an objection to working the furnace with so light a burden of vat waste and burnt pyrites as this involved, because then a portion of the reduced iron made its appearance, not as sulphide, but as white pig metal. Occasionally, indeed, the metallic iron constituted the larger portion of the cast, so that I have little doubt, had the necessary precautions been taken, the whole of the metal contained in the pyrites could have been obtained in this form. Two samples of this white iron were examined and found to contain 2 and 2·9 per cent. sulphur respectively, which, although more than ten times as much as should be present in pig iron, is not an extraordinary quantity to exist in metal running from a furnace in which every cwt. of iron was accompanied by above half a cwt. of actual sulphur.

To secure the conversion of the iron into a sulphide, it appeared indispensable to work the furnace at a comparatively low temperature, which necessitated, therefore, the loss of a certain quantity of the iron being carried away in the slag, and this iron was accompanied by a certain portion of sulphur.

The slag, under these circumstances, was black and perfectly vitreous on being suddenly cooled. On the whole, it was tolerably uniform in composition, and, excepting in point of iron and sulphur, was analogous to that produced at an iron furnace.

The following analyses give the proportions in which the constituents were found in different specimens :—

Silica	40·00	36·8	46·	44	40	44
Lime	27·7	35·6	24·4			
Alumina	18·	23·2	15·6			
Iron	8·9	1·4	7·6			
Sulphur	4·9	4·5	2·4	2·2	2·2	2·3
	99·5	101·5	96·00			

Upon one occasion, for several consecutive days, the sulphur was under 3 per cent. in the slag, and the iron from 2·2 to 2·7 per cent.

FACTITIOUS PYRITES.—When freshly fractured this had often a deep bronze colour, a metallic lustre, with a somewhat crystalline structure; at other times it had a granular appearance, and was of a darker colour. Externally, it was always black, the result of the action of the atmosphere upon it while in the liquid state.

The irregular character of the working of the furnace already mentioned, caused, at certain periods, considerable fluctuations in the character of the resulting sulphide, nevertheless, we did obtain for many days consecutively the product with a tolerable uniformity of composition. In proof of this, I annex the sulphur assays for a fortnight's workings.

31·%—32·5—31·8—33·5—31·—33·2—25·5—28·—33·2—27·2—29·0—30·—31·5—31·5—Average 30·63.

The conditions which we were obliged to observe in working the furnace were such that not only did a portion of oxide of iron find its way into the slag but the factitious pyrites itself was never free from oxygen. A sample analyzed by Mr. J. Glover gave—

Iron	65
Sulphur	31·8
Oxygen	4·1
	100·9

Calling the sulphur two equivalents (32) and assigning to it two equivalents of iron, 56 of the 65 will be absorbed, leaving 9 of iron to be associated with 4·1 of oxygen, which is in the ratio of ·32 of an equivalent of iron to ·51 of an equivalent of oxygen. These proportions are as nearly as may be those in which these two elements exist in $Fe_2\ O_3$.

Never having been able, from actual workings, to estimate the exact proportion of our make of vat refuse dealt with in this furnace, I am

ible to speak confidently on the proportion of sulphur recovered in
factitious pyrites, but reference to estimates made at the time
ves the following results.

The vat refuse was considered to contain 20 per cent. of moisture, ad in its dry state to consist in whole numbers of

Coke and Sand	29		
Sulphur	17		
Calcium	20	= Lime	28
Lime	17	= „	17
Carbonic Acid	12		
Soda, &c.	5		
	100		45

From which it will be observed that the 37 of Ca and CaO make their appearance as 45 of CaO in the slag.

The burnt pyrites was considered to consist of—

Iron	55
Sulphur	6
Silica, &c.	18
Oxygen	21
	100

The clay was of various composition, and although containing some iron, was regarded as going wholly into the slag.

The coke was generally assumed as being composed of 90 of carbon and 10 of ash.

These substances were used in the following proportions—4 cwts. of coke, 3 cwts. of soda waste, 1½ cwt. of burnt pyrites, and 2 cwts. of clay, and their constituents were disposed of in the following way:

	Volatilized.	Slag.	Factitious Pyrites.
Coke.....448 lbs.	Carbon.....404	Ash 44	
Vat waste ...336 „	Water 67	Lime ...121	
equal to 355 when	Carbon 60	Sand ... 17	
the Ca is oxidized.	Carb. acid... 32	Soda, &c. 13	
	Sulphur 10	Sulphur... 7	Sulphur...28
Burnt pyrites 168	Oxygen..... 30	Sulphur..10	Oxygen ... 5
		Iron.....35	Iron 58
		Silica ...30	
Clay.....220 lbs.		Clay.....220	
	603	497	91

These figures give a factitious pyrites of 30·7 per cent., and as the vat refuse employed contained 45 lbs. of sulphur, and 28 are returned in the product, something like 62 per cent. of the sulphur in the waste was thus recovered.

The remaining 17 of sulphur in the waste, along with 10 in the burnt pyrites, have disappeared, and as the slag did not contain generally much above 4 per cent. the total quantity was considered as accounted for in the following manner :—

There went to form Factitious Pyrites	28
to form Slag	17
Disappeared as Vapour	10
	55

In the event, however, of the slag having its iron reduced to 2½ per cent., we should have had 23 lbs. more iron to go to form factitious pyrites, or 81 lbs. instead of 58, and the weight of the sulphide would have been as follows :—

Iron	81 lbs.
Sulphur	40 „
Oxygen	5 „

So, that in such a case, 88 per cent. of the sulphur originally existing in the vat waste would be recovered. In this last estimate the sulphide is considered as yielding 32 per cent. of sulphur, but we have upon several occasions obtained it containing as far as 34 per cent.

When it is remembered this was a branch of manufacture which never emerged from a state of infancy, it seems only reasonable to suppose, that we might have ultimately secured results which would have been constant in returning something like 90 per cent of the sulphur now conveyed to the spoil heap.

Unfortunately, however, for its growth into a state of maturity, the difficulties already given do not exhaust the catalogue of the troubles we had to encounter. The factitious pyrites had to be burnt so as to obtain the treasure it contained, and when we came to employ it as a substitute for native pyrites, the closeness of its texture rendered it a very obstinate material to deal with.

It is not a very easy matter to convey in numerical results a correct idea of the want of success which attended our first efforts in

burning this factitious sulphide. Used by itself in an ordinary pyrites kiln I much question whether one-half the sulphur would have been obtained, if, indeed, we could have maintained for any length of time a sufficient temperature to continue combustion. To assist in keeping up the heat of the kilns, it was usually burnt along with coal-brasses or native pyrites, and I give you the result of some experiments in the hand writing of our colleague, Mr. John Glover, who shared with me, at the time, the anxieties of the undertaking.

A kiln was charged with 3½ cwts. of the artificial sulphide and 1 cwt. of coal-brasses, and from the burnt residue there was selected a large number of pieces about the size of pigeons' eggs and others as large as hens' eggs.

Of the former	35	per cent. contained	5	per cent. of sulphur.	
„	38	„	13	„	
„	27	„	21·7	„	
being an average of 12·54 per cent.					
The larger pieces gave	16	per cent. containing	14·6	of sulphur.	
„	50	„	18·6	„	
„	34	„	23	„	

It is obvious that this was a state of things which could not be persisted in, and I was led to adopt an entirely different plan of treatment.

I had observed that several of the lots of factitious pyrites when exposed to the action of the atmosphere crumbled down into fine powder. I regret we were too much occupied in other more pressing matters to fully investigate the cause of this action. My own impression, but it is only an impression, is that it was due to sodium reduced from the soda always present in the vat refuse. This is certain that the crumbled mass always yielded traces of sulphide of sodium.

Guided by this observation respecting the properties of the factitious pyrites, instead of running it into masses as was formerly practised, we received it in water, which reduced the whole into the condition of a fine slime of a deep black colour. This was removed and laid upon a drying hearth, and possessing coherent properties when dry, it was obtained in the form of cakes so solid and hard as to have a semi-ring when struck with a hammer.

These cakes consisting of the agglomerated slime exposed now so great a surface for oxidation that they not unfrequently took fire on the drying hearth, which was one used for drying common bricks, and on which the temperature never reached 212° Fahr.

In attempting their combustion in an ordinary kiln, from which the burnt material is drawn at the bottom while fresh is introduced at the top, some inconvenience was experienced in the crushing down of the cakes or balls. To avoid this, large vaults were constructed into which 20 to 30 tons of the sulphide was piled. Fire was communicated at the top so that as the sulphur was burnt off a sufficiently elevated temperature was maintained in the partially exhausted pyrites by the heat ascending from the portions below in which combustion was at its height. In two to three weeks the operation was complete, when the oxidized material was removed and returned to the blast furnace to furnish iron for a fresh quantity of factitious pyrites.

I find the particulars of one careful examination of such a charge given. The cakes were piled six or seven feet high in the kiln, which in structure closely resembled those generally employed in burning lime for making bleaching powder. The mass was divided horizontally into three portions, and average specimens taken from each by grinding a considerable quantity.

The uppermost portion contained only 3 per cent. of sulphur and the lowest 6 per cent., the intermediate one giving about the mean of the other two; and what is worthy of notice is, that by far the greatest portion of this sulphur existed as persulphate of iron; indeed, in the case of the top layer, the sulphur was almost wholly in this condition.

A considerable quantity of the artificial sulphide was treated in this manner, with such results as to justify a confident belief that there would be no difficulty whatever in employing it successfully as a substitute for pyrites, so that the only remaining obstacle to success would be the somewhat irregular action of the blast furnace itself.

Unfortunately, however, for the realization of our hopes of ultimate success, the fall in the market value of the native pyrites so reduced the margin of profit that the process was abandoned, after at the Washington and Felling works, something like 4000 to 5000 tons of the factitious pyrites had been made.

Should any circumstance lead to a rise in the price of the natural sulphide, I have no doubt the process, of which I have given a slight account, may be profitably applied for recovering a larger proportion of the sulphur consumed in the manufacture of soda than seems to be attainable by any other mode of procedure hitherto suggested, and without requiring the assistance of any material not within easy reach in respect to cost.

In the event of the assistance of such a mode of recovering sulphur as that described being required, I can only hope that my experience, and the paper just read, may assist the labours of those on whom the direction of our soda works must hereafter devolve.

The PRESIDENT—This is a very important question for soda makers, and perhaps Dr. Lunge may have something to say upon it.

Dr. LUNGE—Before complying with the request of the President, I will take the liberty of asking a question. I have always experienced great difficulty in reading technical books containing accounts of unsuccessful experiments, because they were in fact more than that, inasmuch as there were accounts of experiments which were not mentioned as unsuccessful, but which I never heard of as having been actually carried out. In technical books there are splendid methods for utilizing everything, but when you come to manufacturing districts it is found that they cannot be carried out. With regard to one process which was described as quite successful, when I tried it I found that it was a complete myth, and had never been actually working. In a very good technical book published last year, I found a process mentioned as having been carried out on a large scale in Lancashire, which really stopped after about two months' working. A similar difficulty meets me now, for I must say that after hearing the extremely lucid and complete statements of the President and Mr. Glover, I cannot quite see why this factitious pyrites should not have paid to make when the price was tenpence per unit for all the better kinds of pyrites. Mr. Bell has stated that the cost of factitious pyrites was sixpence per unit, so that it is not quite clear to me that when the price of ordinary pyrites exceeded sixpence, it was not possible to use it then. From the paper read last year by Mr. Mond, it will be known that his process had been carried

out at the chemical works with which I am connected. I am bound to say that the results of the process in my hands have, in one respect, not been so satisfactory as he anticipated they would be, namely, as regards the yield of sulphur from the waste. The actual expense is not great. I have not found Mr. Mond wrong with respect to the labour connected with the process, for it is very small, and the amount of coal is smaller than he said it would be, owing to the employment of a steam apparatus for melting the sulphur. He did not include the steam apparatus in his original process, simply because Mr. Schaffners' patent was still extant; but since that patent has become void, Mr. Mond has adopted the steam apparatus himself, and I have simplified it so that at the present moment the sulphur costs me very little. I think that, apart from the royalty, the cost of the sulphur does not exceed the 25s., but I do not get half of what Mr. Mond promised. I know that many people recover more than I do, but the cause of this may be that the process is different here to what it is in Lancashire, where they work with limestone, whereas here they work with chalk, and possibly our waste was more disposed, in consequence of that, to a thorough oxidation of the sulphur forming sulphate of lime. The precipitate of sulphur which I obtain by mixing clear sulphur liquor and muriatic acid contains great quantities of sulphate of lime, but after melting it contains never more than 0·02 to 0·05 per cent. impurities. I have had it as pure as 99·98, and it was perfectly free from arsenic. It is only the operation of melting that separates the sulphate of lime from the sulphur. There might be a little sulphuric acid in the muriatic acid which I used; but, at all events, it could not account for anything like the enormous quantity of sulphate of lime which was actually formed. I am sorry I have not had time to inquire fully into the matter. Mr. Mond supposes that certain sulphur salts which are present in the liquors, on decomposition, might form some sulphate of lime. The great desideratum, however, namely, avoiding the waste of the most costly produce we carry into our works, is not attained in anything like a satisfactory way; the only thing fully achieved by Mr. Mond's process is the removal of the noxious properties of the waste.

Mr. GLOVER—What do you mean by the enormous amount of sulphate of lime which accompanied the sulphur? Can you give us any figures?

Dr. LUNGE—The bulk of the sulphate of lime blown out of the apparatus often considerably exceeded that of the melted sulphur. I think it may be about 30 per cent., and if it will be of any interest I will give a sketch of the melting apparatus which he employed. Dr. Lunge having been requested to do so, proceeded to explain Mr. Schaffners' apparatus, and also Mr. Mond's apparatus. In answer to the President, he said the pressure was 30 lbs.

Mr. PATTINSON—Did I understand you to say that in your experience of this process it does not pay?

Dr. LUNGE—My experience, as far as it has gone, is that it pays perfectly well. The sulphur costs so little that it always pays perfectly. It does not make as much as I was led to expect.

Mr. PATTINSON—It does not pay to the amount Mr. Mond promised?

Dr. LUNGE—He held out the hope that we should make quite three times as much, and that it would not cost any more. In answer to Mr. Glover, we did not get more than about one-fifth of what we knew was in the waste. I have gone most carefully through every stage of the process, and have found everything to be up to the standard Mr. Mond had put before us. We get correct results, except in the important element of quantity.

Mr. PATTINSON—I would like to express my personal gratification at seeing the results of Mr. Bell's experience, more especially as I watched the operation of the experiments at the Felling, simultaneously with those going on at Washington. It is, therefore, extremely gratifying to me to find that these results have been placed on record.

The PRESIDENT—In reply to Dr. Lunge, I may observe that having been able to purchase pyrites at the low rate named, we were compelled to relinquish the process, and although that mineral occasionally rose to a much higher price than this, the market never presented such an appearance as to encourage us to encounter the expense and difficulties of returning to the manufacture of this

artificial sulphide. But the time may come when pyrites may rise in price; and it is perhaps useful to know that we shall then be enabled to procure artificial pyrites.

On the motion of Mr. PATTINSON, seconded by Mr. BRADY, a vote of thanks was passed to the President for his paper.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, JANUARY 27, 1870.

MR. JOHN GLOVER IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Mr. Wm Blench and Mr. T. Brayshay were elected members of the Society.

Mr. JOHN PATTINSON read a paper

ON THE ESTIMATION OF PEROXIDE OF MANGANESE IN MANGANESE ORES.

IT has long been known that very varying results have been obtained by different analysts in operating upon the same samples of manganese ore, and, very naturally, these discrepancies have been a source of great annoyance to both sellers and purchasers of these ores. In their attempts to find the cause of these differences Messrs. Teschemacher and Smith were led to the discovery that many manganese ores now in the market contain magnetic oxide of iron. It is said that this substance appropriates a portion of the peroxide of manganese in the ore when the sample is tested by what is known as the iron method of estimation, but not when tested by Fresenius and Will's oxalic acid method; in other words, that the iron process indicates only the *available* amount of peroxide of manganese in the ore, and the Fresenius and Will process the *absolute* amount; and hence, they say, the differences in the results found by the various analysts who use these two methods. This

theory may possibly be correct in some cases, but it is certain that this is not the only reason why the Fresenius and Will process gives higher results than the iron process in testing certain ores. I have met with several samples of manganese ore containing considerable quantities of magnetic oxide of iron, but I have never yet met with one the magnetic oxide of which was not fully oxidized at the expense of the peroxide of manganese of the ore on being tested by Fresenius and Will's method.

Before proceeding further it will be desirable to consider what takes place when the Fresenius and Will test is applied in the presence of protoxide of iron. If the protoxide of iron is in the soluble state it is entirely peroxidised by the oxygen of the peroxide of manganese, and the test indicates a relatively less amount of peroxide in the ore examined. This was proved by the following experiment:—30 grs. of a soft and easily-decomposed manganese ore free from magnetic oxide and known to contain by the Fresenius and Will test 63·36 per cent. of peroxide, together with 70 grs. of neutral oxalate of potash, 20 grs. of proto-sulphate of iron containing 3·90 grs. of iron in the state of protoxide, and some water, were treated in the usual way according to the Fresenius and Will method. The percentage of peroxide indicated by the loss of carbonic acid was 53·13, showing that the protoxide of iron in the proto-sulphate had appropriated 10·23 per cent. of peroxide. Theory requires 10·21 per cent. of peroxide to effect the oxidation of all the iron. The effect is different when the protoxide of iron in the ore is in the insoluble state, as, for instance, when magnetic oxide is present. In this case the protoxide is only peroxidised as it becomes dissolved. It is possible that ores contain ing magnetic oxide may exist which have their peroxide in a conditio in which it is so easily decomposed by oxalic acid that the whole m be decomposed before the magnetic oxide has been at all dissolv In this case the test would indicate the *absolute* amount of pero in the ore. That such a condition may obtain is seen by the follo experiment:—25 grs. of an ore similar to that used in the last ex ment but containing 63·60 per cent. of peroxide by both the iro oxalic acid tests, together with 60 grs. of neutral oxalate of 5 grs. of very finely-powdered magnetic oxide of iron known tain 18·6 per cent. of iron as protoxide, and some water, wer

in the Fresenius and Will apparatus. On testing in the usual way, the loss of carbonic acid indicated 63·50 per cent. of peroxide, or within 0·1 of a per cent. of what it was without the magnetic oxide. The magnetic oxide, however, was apparently but very slightly, if at all, affected. A further quantity of sulphuric acid was then added, and heat applied until the solution of the magnetic oxide was completed. The amount of iron existing in the solution was then determined by a standard solution of bichromate of potash. It indicated 1·08 grs. of iron in this condition. The amount in the five grains of magnetic oxide was 0·93 grs. The excess is accounted for by the fact that the oxalic acid in the solution somewhat interferes with the bichromate of potash test for iron.

I have said it is possible that ores of the character described above may exist—ores in which the Fresenius and Will test will indicate the absolute amount of peroxide they contain; but I have never met with such ores. Those I have seen containing magnetic oxide of iron—and some samples have contained a considerable amount—have had this substance so intimately diffused throughout the ore, and have had their peroxide in a condition so difficult to decompose, that the magnetic oxide has been quite dissolved before, or as soon as the complete decomposition of the peroxide has been effected. This was easily proved by testing the resulting solution of manganese and iron for protoxide of iron by means of a few drops of a solution of red prussiate of potash added to the diluted solution. It is obvious that, if after complete decomposition of the ore, the manganese has not peroxidized the whole of the protoxide of the magnetic ore, its presence would be detected by the above test. *In no case have I found any protosalt of iron in the resulting solutions.* Therefore, in all the cases I have met with, some of the peroxide of the ore has been appropriated to completely peroxidize the magnetic ore, and the Fresenius and Will test, in these cases, does *not* indicate the absolute amount of peroxide contained in the ores.

In my opinion, the more likely explanation of the cause of the discrepancies between the various tests is to be found in the great difficulty with which these hard ores containing magnetic oxide of iron are decomposed in the Fresenius and Will apparatus. In order to effect their decomposition it is necessary to add a large quantity of

sulphuric acid, and to apply extraneous heat for a considerable time. It is probable that, during this lengthened heating, steam is allowed to escape with the carbonic acid, and hence a higher amount of peroxide is indicated than is present in the ore.

But whichever theory is correct, both lead to the same conclusion—that with such hard ores the Fresenius and Will test gives inaccurate results, and therefore should be abandoned. For soft and easily-decomposed ores I believe its indications are very correct; but as several of the other class of ores are now in the market, it is necessary to have some process by which the available peroxide contained in all kinds can be correctly determined.

The iron process is one which is now in very general use, and has usually been found to give very good results.

In the paper "On the Estimation of Peroxide of Manganese in Manganese Ores," by Messrs. E. Sherer and G. Rumpf, read before the members of this society at their meeting in November last, these gentlemen show that they had obtained inaccurate and irregular results in their experiments with this method as followed by them. They had used pianoforte wire dissolved in a flask with hydrochloric acid, adding the manganese ore in a glass tube, and taking the precaution of keeping an atmosphere of carbonic acid in the flask during the whole process to prevent oxidation by atmospheric air. They attributed the irregularity of the results they had obtained, and correctly I think, to the circumstance that portions of the manganese ore are occasionally floated to the surface of the solution where they evolve chlorine, which escapes without coming into contact with the protosalt of iron in solution. That this process can be made to yield correct results, by taking precautions to prevent this escape of chlorine, the authors themselves have shown. Fresenius also states in his handbook that the results he obtained by this process agreed exactly with those obtained by that of Fresenius and Will. Messrs. Sherer and Rumpf come to the conclusion that, of the methods they experimented with, Bunsen's gave the most correct results, and that this method was much simpler and shorter of execution than the iron method when the necessary precautions to insure accuracy were employed in the latter process.

In the discussion which followed the reading of this paper I stated

that, being aware of the source of error the authors had pointed out, I had, for some time past, used in my laboratory a modification of the iron process by means of which the source of error was avoided, and perfectly accurate and concordant results obtained; and that the modified process was, in my opinion, more easy of execution—requiring less skill and care at the hands of the operator—than the Bunsen method of estimation. In fulfilment of a promise I then made I have now to describe the manner in which I proceed when testing manganese ores by the iron method.

30 grs. of clean iron wire are placed in a 20 oz. flask, along with 3 ozs. of dilute sulphuric acid, made by adding 3 parts of water to one of oil of vitriol. A cork, through which passes a tube bent twice at right angles, is inserted in the neck of the flask, and the flask is heated over a gas flame until the iron is dissolved. The bent tube is placed so as to dip into a small flask or beaker containing a little water. When the iron is quite dissolved, 30 grs. of the finely-pounded and dried sample of manganese ore to be tested are put into the flask, the cork replaced, and the contents again made to boil gently over a gas flame until it is seen that the whole of the black part of the manganese is dissolved. The water in the small flask or beaker is then allowed to recede through the bent tube into the larger flask, more distilled water is added to rinse out the small flask or beaker and bent tube, the cork well rinsed, and the contents of the flask made up to about 8 or 10 ozs. with distilled water. The amount of iron remaining unoxidized in the solution is then ascertained by means of a standard solution of bichromate of potash. The amount the bichromate indicates, deducted from the total amount of iron used, gives the amount of iron which has been peroxidized by the manganese ore, and from which can be calculated the per centage of peroxide of manganese contained in the ore. Thus, supposing it were found that 4 grs. of iron remained unoxidized, then 30—4=26 grs. of iron which have been oxidized by the 30 grs. of ore. Then, as

Iron.		Peroxide of manganese.		Iron.		Peroxide of manganese.
56	:	44	::	26	:	20·43

the amount of peroxide in the 30 grs. of ore. The percentage is, therefore, 68·10.

It will be seen, from the above description, that the only differences between this method and that usually described in hand-books are the use of sulphuric acid instead of hydrochloric acid, and the dispensing with the use of a stream of carbonic acid. These alterations, however, enable us to get rid of the source of error indicated by Messrs. Sherer and Rumpf, and also of the somewhat complicated and troublesome apparatus required to supply a stream of carbonic acid.

In order to determine the accuracy of this method, some samples of soft and easily-decomposed manganese ore known to be free from magnetic oxide, were tested by Fresenius and Will's method, and compared with the results obtained by the iron method. The results obtained were as follows :—

By Fresenius and Will's Process.	By Iron Process.
63·36 per cent.	63·43 per cent.
82·34 „	82·34 „
74·00 „	73·98 „
63·32 „	63·25 „

That by the iron method results can be obtained which agree very closely with each other will be seen by the following statement:—Of 85 samples tested in duplicate by this method 22 were exactly the same in the two results, 54 were within 0·1 of a per cent. of each other, 8 were within 0·2 of a per cent., and 1 differed 0·2 of a per cent. in the two tests.

The use of sulphuric acid instead of hydrochloric acid not only gets rid of the before-mentioned source of error, but it is also the reason why the carbonic acid atmosphere can be dispensed with. I have ascertained that the solution of iron in sulphuric acid does not oxidize appreciably by exposure to air within any reasonable time; certainly not within the time required to make the test. A sample of manganese ore tested after allowing the iron solution to cool in an atmosphere of carbonic acid, and taking precautions to prevent the admission of air, indicated 63·98 per cent. of peroxide by two separate tests. Another portion of the same ore was tested after another portion of iron had been simply allowed to cool in air and to stand one hour before adding the manganese ore. It indicated exactly the same as the sample cooled in carbonic acid, viz. :—63·98 per cent. Another sample, after the iron solution had been allowed to stand two hours

exposed to the air, still indicated 63·98 per cent. of peroxide. After 19 hours' exposure of another portion the ore indicated 63·96 per cent. There was, therefore, no practical difference in the amount of iron existing as protoxide in any of these solutions. In another experiment a sample of manganese ore, which had indicated 64·45 per cent. of peroxide on cooling the iron in carbonic acid, indicated 64·50 per cent. after the iron solution had been exposed to the air for six hours, and 64·69 per cent. after the iron solution had been exposed to the air during 48 hours. In the last case the iron had become oxidized to a slight extent, but in the case of the iron exposed for six hours little or no oxidation had occurred.

The same results have been repeatedly obtained in other experiments.

The solution of iron in hydrochloric acid oxidizes much more rapidly. 7 grs. of iron wire dissolved in hydrochloric acid, and allowed to stand 18 hours exposed to the air in a flask with a cork through which passed an open tube, indicated 6·2 grs. of iron existing as a protosalt; whilst 7 grs. dissolved in sulphuric acid, and exposed for the same length of time, under the same circumstances, indicated 6·995 of iron existing as a protosalt.

The kind of iron wire I have found to answer best for these analyses is the kind used by wire workers, and known as "annealed iron wire." Only the purest qualities of iron are used for wire of this description. I have carefully examined it by comparing it with pure oxalic acid and pure bichromate of potash, as well as by heating it in an atmosphere of chlorine, and find that it is within about 0·1 of a per cent. of being absolutely pure. For all practical purposes of testing it may, therefore, be considered pure. There is no difficulty in obtaining it in large quantity and of uniform quality. I have tested wire of this kind of such thickness that 22 wires laid close together cover an inch, and I find it of equally pure quality as that which requires about 85 wires to the inch. I prefer the latter because it is more easily cut and dissolved, and it is more likely to be pure. Pianoforte wire, which is made of steel, will not do, excepting a correction is made for the amount of carbon it contains. I have found wire of this kind which contains only 98·50 per cent. of pure iron. The odour of the hydrogen evolved during solution is a good

indication of the purity of the iron. With steel wires the odour of carburetted hydrogen is very strong, but with the annealed iron wire it is very slight. In all cases it will be desirable to ascertain the purity of every new batch of iron wire obtained previous to its being used for testing manganese ores. Having once obtained wire of known purity, all future batches may be most easily and correctly tested by comparing the amount of peroxide of manganese, and, consequently, of iron, indicated by a certain ore when tested by the new wire with the amount indicated when tested by the wire of known purity. The wire is easily cleaned from all adhering oxide and dirt by passing it a few times through fine emery paper and afterwards through a cloth. It is dissolved by the sulphuric acid in about fifteen minutes. I do not like to use crystals of protosulphate of iron, nor of the double sulphate of iron and ammonia, on account of the uncertainty of their composition. I have never yet met with either of these in so pure a condition as the iron wire. Besides there is no advantage in their use. It is quite as easy to weigh the iron wire as to weigh crystals of protosulphate of iron. I have tried a standard solution of protosulphate of iron, but find it does not keep sufficiently well to make it worth while to use this.

The use of the small flask or beaker in connection with the bent tube is to prevent all loss of iron by its being carried over with the hydrogen and steam. I have occasionally found a slight trace of the iron thus carried over through the tube, but this is, of course, all arrested by the water in the small beaker or flask, and is again returned into the larger flask when the tube is rinsed out.

The length of time required to decompose the manganese ore depends upon the hardness of the sample under examination, and this mode of testing thus affords a good indication of the nature of the ore in this respect. Soft ores will be decomposed in a minute or two, whilst very hard ores require fifteen minutes or more. The decomposition can be hastened by adding more oil of vitriol, but this must be done after the iron is dissolved, as the latter does not readily dissolve in stronger acid than I have mentioned.

The standard bichromate of potash solution I use is made so that 1,000 fluid grains will peroxidize 10 grains of iron. It is applied to the diluted solution in the usual way.

If there is reason to suppose that the ore to be tested contains more than about 78 per cent. of peroxide, it will then, of course, be necessary to use less than 30 grs. of ore or more than 30 grs. of iron; as these quantities are only intended for ores of a lower percentage than about 78.

In order to facilitate and check the calculation of the percentage results, I have a table drawn up which shows the various percentages in the ores corresponding to every five grains of the bichromate solution, based upon the assumption that 30 grs. of iron and 30 grs. of ore are used.

In conclusion, I have to say that, with respect to the Bunsen method of estimation, it has given me unsatisfactory and variable results so far as I have tried it. I have, however, not yet completed my investigations into this method.

The CHAIRMAN—We shall be happy to hear the observations of any gentleman, who may favour us with his thoughts on the subject, after hearing the very able, and, I may say, somewhat exhaustive paper of Mr. Pattinson.

Mr. C. R. A. WRIGHT—I have listened with great pleasure to the able and interesting paper that has just been read by Mr. Pattinson; and the more, that his results entirely corroborate those formerly obtained by myself ("Chemical News," Oct. 4th, 1867), viz., that identical results are obtainable by the iron process and that of Fresenius and Will, at any rate in the vast majority of instances; and that the presence of soluble ferrous compounds causes the amount of carbonic acid obtained by this latter method to represent, not the total Mu O_2 present, but that left after peroxidation of the iron present, just as in the case when Bunsen's process, or others of similar character, is adopted. In Mr. Pattinson's improved iron process, apparently the only remaining source of inaccuracy lies in the sulphuric acid used; even so-called "pure" acid frequently contains sufficient arsenic to make a perceptible increase in the amount of bichromate (or permanganate) solution required for the back titration of the iron; the arsenic exists as arsenious oxide, and becomes converted into arsenic acid either by the manganese, or by the bichromate (or permanganate), thus diminishing to a certain extent the apparent quantity of Mu O_2 present.

Dr. LUNGE—I regret I was not present when the greater part of Mr. Pattinson's paper was read. I cannot, therefore, make any remark upon it. I would, however, like to ask Mr. Wright, if in his remarks at the discussion on Mr. Sherer's paper, he gave his own experience, or only what he had found in books relative to the permanency of the alkaline solution of arsenious acid? My own experience has proved that the solution keeps unaltered for a very lengthened period, without taking any special precautions. I used to prepare it and keep it in large bottles from which the burettes were filled until all the solution was used. I have always found that it contained as much arsenious acid at the end as at the beginning. No further precautions are necessary than are required for keeping other solutions, such as bichromate of potash.

Mr. WRIGHT—I have not experimented with the alkaline solution of arsenious acid. With regard to the question which Dr. Lunge has asked, I have to say, that I had seen it stated that it did not keep,* and knowing that the acid solution kept well, I always used this in my experiments, and rendered it alkaline immediately before doing so.

Dr. LUNGE—No chemist of any note has made any experiments which showed that the arsenious acid solution changes by lengthened exposure to atmospheric air.

Mr. J. W. SWAN—With reference to the preservation of protosulphate of iron, I have found—and the fact, perhaps, is not so widely known as it ought to be—that camphor has a wonderful effect in preventing the oxidation of the iron.

The CHAIRMAN—That refers to the crystals?

Mr. J W. SWAN—Yes, and it might probably have the same effect upon the solution.

Dr. LUNGE—I have seen camphor recommended in order to preserve crystals, but I have never seen anything with respect to its application to the solution.

Mr. JOHN PATTINSON—With reference to the remark of Mr. Wright that arsenious acid, sometimes present in sulphuric acid, would affect the accuracy of the results, I have to say, that it is taken for granted, that pure sulphuric acid should be used. I have,

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* See Annal. d. Chem. u. Pharm., vol. 93, p. 384; vol, 94, p. 222; and vol. 101, p. 266.

however, found that small quantities of sulphurous and nitrous acids present in the sulphuric acid used do not materially affect the accuracy of the manganese test; and for this reason, these acids are generally completely expelled or decomposed before the whole of the iron is dissolved, and consequently before the manganese ore is added, the whole of the iron still existing in the state of protoxide. But I, of course, always use pure sulphuric acid. Mr. Swan's suggestion as to the use of camphor, to keep the standard solution of proto-sulphate of iron from being oxidized may be worth trying. In chemical manufactories where economy of time is of greater importance than strict accuracy, the standard solution of proto-sulphate of iron would be useful: but I do not like the measuring out of the solution of iron. I have frequently known errors to arise in doing this, owing, probably, to the small quantity of the iron solution necessarily used for standardizing as compared with the large quantity used in the manganese test. I always prefer to use pure iron wire. The weighing and dissolving of this do not require much time. With reference to the question of the stability of the alkaline solution of arsenious acid used in testing bleaching powder, my experience quite corroborates that of Dr. Lunge. I have experimented on this point and I have found that the solution keeps unchanged for a considerable length of time.

Mr. WRIGHT—In the old original apparatus of Fresenius and Will, two flasks were connected together by about six or eight inches of glass tube, which was necessarily kept moistened with sulphuric acid, and the carbonic acid gas in passing through this tube was more likely to become deprived of its moisture than when it is simply passed through an inch or so of sulphuric acid. There is another point, and that is that the manganese ore must be extremely finely pounded. Fresenius recommended a very free use of the agate mortar, freer than it always gets I imagine. The iron wire employed by Mr. Pattinson appears, practically at any rate, to be perfection; I have purchased soft iron wire, however, not containing more than 97 per cent. of iron.

Dr. LUNGE—I perfectly agree with Mr. Pattinson, that it is not desirable to use proto-sulphate of iron, and certainly not when pure iron wire of the kind described by Mr. Pattinson can be readily obtained.

The CHAIRMAN—As to the probability of water vapour passing through the sulphuric acid with the carbonic acid, I, some time ago, experimented on the condensation and absorption of hydrochloric acid. I had to pass it through a series of six Woulfe's bottles, containing water, before I could get the whole of it absorbed. I think from the experiments I have mentioned, and the difficulty found in absorbing hydrochloric acid, that it is quite possible that aqueous vapour might pass through the sulphuric acid with the carbonic acid liberated in the Fresenius and Will's test, as applied to the hard ores of manganese. In moving a vote of thanks to Mr. Pattinson, I think we shall all agree that he has given us a most able, and so far as it could be, a most exhaustive paper.

Mr. WRIGHT seconded the motion, which was passed by acclamation.

Mr. JOHN PATTINSON, in returning thanks, said—I shall be amply repaid for any trouble I may have taken in preparing this paper if it be the means of preventing the discrepancies we have hitherto known in the testing of manganese ores.

The CHAIRMAN—I see so many chemists present, and many young men who have a name to make that I would respectfully suggest to them that they should prepare papers, short or long it does not matter which, for reading at these meetings. You are all aware that truth is evolved by discussion, and although we might record opinions and experiments with which many persons might not agree, still the discussions would be beneficial to themselves and the society, because it is only by the clashing of opinions we shall be likely to evolve the truth.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, FEBRUARY 24, 1870.

MR. R. C. CLAPHAM IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

The name of Mr. John Mitchell, Monkton, was read for the first time.

Mr. WRIGHT read,

NOTE ON THE ACTION OF IODINE ON HYPOSULPHITE.

IN a paper on the valuation of manganese ores, read before the Newcastle Chemical Society, on November 25th, 1869, Messrs. Sherer and Rumpf state that they find it necessary to titrate the iodine solution obtained by Bunsen's process with standard hyposulphite solution "as soon as possible after the decomposition, since the solution, in presence of air, gradually reacts on the iodide of potassium and liberates iodine;" the iodine solutions in each of two experiments requiring an amount of hyposulphite corresponding to 62·7 per cent. of available peroxide in the manganese ore assayed when tested immediately after the decomposition, and to over 65 per cent. after standing twenty-four hours; Fresenius and Will's process giving, with the same sample, the intermediate number, 63·5 per cent.

In order to see how far this discrepancy might be accounted for by a possible irregularity in the action of iodine on hyposulphite under different circumstances as to temperature, &c., a solution of iodine in potassium iodide was prepared of the strength of 5 grammes free iodine to the litre; a solution of pure recrystallized sodium hyposulphite was also prepared.

Into each of eleven flasks 25·65 Ccs of this hyposulphite was

measured by a pipette delivering that amount; about 200 Ccs of water was added to each. In the first flask the quantity of iodine solution requisite to produce a blue tint along with starch, at the ordinary temperature of the air (16°C) was determined; the second flask was heated to about 60°C, then allowed to cool, and finally titrated as before; almost the same amount of iodine being required, it appears that simply heating the hyposulphite solution does not appreciably alter the quantity of hyposulphite present.

The next eight flasks were heated to varying temperatures, and while hot a quantity of iodine solution, rather less than that required for the hyposulphite, as deduced from the first experiments, was added, care being taken to run the iodine direct into the warm liquid, and not down the sides of the flask, which might cause a loss of iodine by volatilization; no smell of iodine was observed in any case. The addition of this iodine solution slightly cooling the liquids, their temperatures were again read off, the mean of the two temperatures being taken as the average; the warm liquids were then rapidly cooled by immersion of the flasks in cold water, and finally the slight excess of hyposulphite determined by iodine solution; the cooling was essential, since heat destroys the colour of the iodized starch.

The last flask was again titrated at the temperature of the air (16°C) at the close of the experiments, to make sure that the hyposulphite had not altered spontaneously.

The numbers obtained were—

	1	2	3	4	5	6	7	8	9	10	11
Temperature of solution before addition of iodine solution ..	...	...	32°	40°	52°	55°	86°	88°	100°	100°	
Do., after	...	...	28°	36°	44°	46°	65°	78°	83°	85°	
Mean temperature	16°C	25°	30°	38°	48°	50°	75°	83°	91°	92°	16°
Ccs of iodine required	39·6	39·4	39·8	40·0	40·1	40·1	40·7	40·9	41·4	41·2	39·5
Iodine required in excess of that normally requisite, viz., 39·5 Ccs.....	...		0·3	0·5	0·6	0·6	1·2	1·4	1·9	1·7	
Do., in per cents. of the normal quantity	...		0·75	1·25	1·5	1·5	3·0	3·5	4·75	4·25	

From these numbers it is evident that the higher the temperature the larger is the quantity of iodine requisite before a blue colour with starch is developed, *i.e.*, the action of iodine on hyposulphite is slightly irregular at varying temperatures. Since the first action of iodine is to form tetrathionate (which, as is well known, is converted into sulphate by the action of chlorine), it appears probable that the conversion of tetrathionate into sulphate by iodine may account for the excess required. Probably, therefore, if the mode of experimenting were reversed (*i.e.*, hyposulphite added to warm iodine solution, the latter being always in excess till just the close of the experiment), a greater discrepancy would be manifest. It is difficult, however, to manipulate thus without loss of iodine by volatilization; but it was found that an excess of iodine added to hyposulphite gradually diminished on standing some hours at the ordinary temperature, *i.e.*, less residual free iodine was found after standing than was present immediately after the addition to the hyposulphite.

Thus, 25·65 Ccs of a rather weaker hyposulphite solution than that previously used was pipetted off into each of 10 well stoppered bottles; two were immediately titrated with the former iodine solution, the requisite amount of iodine as thus deduced was added to each of the others; to five of them a varying excess of iodine, and to the remaining three a varying excess of hyposulphite; thus, each of the eight originally contained the same amount of tetrathionate. After standing 24 hours in a cupboard (in the dark) at the ordinary temperature (10° — 20°C,) hyposulphite was added in slight excess to the five in which the iodine was originally in excess, and, finally, all were titrated with the iodine solution. The following numbers were obtained :—

	A	B	C	D	E	F	G	H	I	J
	TITRATED AT ONCE.									
Original excess of Hyposulphite	...	...	...	...	...	...	...	1·5	5·9	9·7
Ditto, Iodine	...	...	0·1	0·2	6·7	16·7	36·7	...	...	...
Total Hyposulphite used	25·65	25·63	26·25	26·4	28·2	31·85	37·85	27·45	31·50	35·25
Iodine equivalent to ditto, (deduced from A and B)	...	...	64·8	65·2	69·6	78·6	93·6	67·8	77·8	87·05
Total Iodine used	63·3	63·35	64·9	65·1	72·7	82·0	101·9	67·9	77·65	87·2
Ratio of Iodine and Hyposulphite used	2·467	2·470	2·472	2·466	2·578	2·574	2·692	2·474	2·465	2·474

From these numbers it appears that in the three last experiments there was no action whatever between the tetrathionate present and the excess of hyposulphite; while in experiments E, F, G, a considerable amount of iodine has *disappeared*, this amount increasing with the original excess of iodine, thus—

	E	F	G
Excess of iodine	6·7	16·7	36·7
Iodine disappeared	3·1	3·4	3·3
Ditto, in per cents. of the normal amount	4·4	4·3	8·9

Similarly, in another experiment with the above iodine solution diluted to one-third its former strength, after 24 hours—

	5 Ccs.	13 Ccs.
Original excess of iodine		
Iodine corresponding to total hyposulphite used.....	163·1	164·4
Total iodine used.....	166·7	169 9
Excess of iodine	3·6	5·5
Do., in per cents. of normal quantity.....	2·1	3·3

On the whole, therefore, these experiments indicate that when the temperature is not lower than 28° C, or when a mixed solution of hyposulphite and iodine still in excess is allowed to stand some time before finishing the titration, a deficiency in the amount of iodine may be expected varying in amount with the circumstances: possibly these facts may account for the results obtained by Messrs. Sherer and Rumpf.

The fact that sulphate is formed by the prolonged action of iodine on tetrathionate is indicated by the following experiments:—50 Ccs of the hyposulphite solution used in the past experiments were mixed with 150 of iodine (large excess); after standing 48 hours at the ordinary temperature in the dark, hyposulphite was added till all free iodine disappeared: chloride of barium gave with this liquid a dense cloud, which after standing was collected, and found by the blow-pipe to be really barium sulphate. 50 Ccs of the original hyposulphite with barium chloride gave not the slightest precipitate; after

addition of a few drops of hydrochloric acid the solution did not become opalescent for a minute or more; and the precipitated sulphur after standing some hours was wholly volatile, showing that no sulphate whatever was present originally in the hyposulphite.

My best thanks are due to Mr. I. L. Bell, in whose laboratory the above experiments were carried out.

ON THE REACTION OF CHLORINE ON SULPHUR SALTS.

The following experiments, made some years ago in the laboratory of Owen's College, under the superintendence of Dr. Roscoe, may be of some interest as bearing on the action of chlorine and its congeners on the sulphur acids.

In order to see how far the action of equivalent quantities of chlorine, bromine, and iodine on the same sulphurous acid solution are comparable, solutions were prepared, one containing five grammes of iodine per litre, and the other such a quantity of sulphurous acid that a litre represented about 150 Ccs of the iodine solution; the exact relation between the two was determined before and after each set of experiments, and it was found that no appreciable alteration took place during the duration of any one set of experiments. A solution of washed chlorine in distilled water was also prepared and kept in the dark, and when required diluted with distilled water to the required strength; the stronger solutions were weighed out in sealed glass bulbs, subsequently broken under the surface of the liquids to be acted on.

A. Known quantities of chlorine solution were added to a measured column of sulphurous acid solution, the latter being in slight excess; this excess was then titrated by the standard iodine solution.

B. Chlorine solution was added to potassium iodide solution, and the liberated iodine treated as the chlorine in *A*.

C. Carried out as *B*, but potassium bromide used instead of iodide.

D. Bromine liberated as in *C*; potassium iodide then added, and the iodine thus liberated by bromine treated as that obtained in *B*.

By subtracting from the iodine solution equivalent to the sulphu-

rous acid used that required for the back titration, numbers were obtained representing the amount of iodine solution equivalent to sulphurous acid destroyed by chlorine, bromine, or iodine in these experiments. Each determination was done in duplicate, frequently in triplicate, and it is noticeable, that whilst the repetitions agreed remarkably closely in all cases where iodine acted on sulphurous acid, they usually showed some little divergence in the case of bromine, and considerably more in the case of chlorine.

Care was taken in all these comparative experiments to have the total volume of liquid in each case as nearly as possible proportionate to the quantity of chlorine originally employed, so that the circumstances of dilution in any set of experiments should be approximately uniform.

The following examples shows the character of the numbers obtained, the same measured volume of chlorine solution containing 0·88 grammes of Cl per litre being used in each instance.

Iodine representing SO_2 destroyed by I, (liberated by Cl and KI.)	Iodine representing SO_2 destroyed by I, (liberated by Cl and K Br, and by Br thus set free and KI.)	Iodine representing SO_2 destroyed by Cl.	Iodine representing SO_2 destroyed by Br.
63·1 Ccs.	62·8	61·6	62·7
62·9	62·6	61·3	62·6
63·1	62·7	60·9	62·7
Mean 63·0	62·7	61·3	62·7

Similarly with other chlorine solutions the following mean numbers were obtained from 18 determinations.

Grammes of Chlorine per litre of solution.	Iodine representing SO_2 destroyed by I (from Cl and KI.)	Iodine representing SO_2 destroyed by Cl.	Iodine representing SO_2 destroyed by Br.
*7·00	49·8	44·5	51·3
0·30	57·0	56·5	56·9

* Nearly saturated Cl solution, containing at 0° 2·2 vols. of Cl gas to one vol. of water.

The above numbers calculated in each case to 100 parts of iodine representing SO_2 destroyed by I (from Cl and KI) are—

Grammes of Cl per litre of solution.	Iodine representing SO_2 destroyed by I (from Cl and KI.)	Iodine representing SO_2 destroyed by I (from Cl and K Br, and from this B and KI.)	Iodine representing SO_2 destroyed by Cl.	Iodine representing SO_2 destroyed by Br.
7·00	100·0	...	89·2	103·0
0·88	100·0	99·5	97·1	99·5
0·30	100·0	...	99·2	99·9

It thus appears that the action of iodine is nearly the same, whether directly set free by chlorine, or liberated by the circuitous process of evolving bromine from chlorine and bromide of potassium, and then causing this bromine to act on iodide of potassium, since the numbers obtained are no more different than might be anticipated from the combined effects of experimental errors, volatility of bromine, &c.; whilst the action of bromine in the two weaker solutions appears to be nearly the same as that of iodine, whereas in the stronger solution a larger quantity of SO_2 is destroyed by bromine than by iodine; lastly, the action of chlorine appears in the weakest solution to be nearly identical with that of iodine, but in the stronger solutions differs considerably therefrom, and this difference, unlike that in the case of bromine, indicates a destruction of *less* SO_2 by chlorine than by an equivalent quantity of iodine.

From a similarly conducted series of 24 determinations the following mean numbers were also obtained as the relative actions of chlorine and iodine solutions :—

Grammes of Cl per litre of solution.	Iodine representing SO_2 destroyed by I (from Cl and KI.)	Iodine representing SO_2 destroyed by Cl.
4·40	100·0	94·7
2·41	100·0	95·4
1·39	100·0	114·4
0·94	100·0	105·4

Experiments were also performed with gaseous chlorine; a known weight of potassium dichromate was treated with hydrochloric acid in

Bunsen's apparatus, and the liberated chlorine led directly into a known slight excess of sulphurous acid solution, subsequently titrated back by the standard iodine; the iodine solution having been originally standardized from the amount of iodine set free by making the chlorine from a known quantity of dichromate act on potassium iodide, the relative actions of chlorine and iodine were directly calculable. In three experiments the following numbers were obtained:—

Grammes of Chlorine evolved per litre of Sulphurous Acid Solution employed	Iodine representing SO_2 destroyed by I (from Cl and KI)	Iodine representing SO_2 destroyed by Gaseous Cl.
0·36	100·0	109·8
0·33	100·0	113·0
0·33	100 0	112·8

It is clear from the above described experiments, that the difference in the action of chlorine and iodine does not depend solely on the strength of the solutions used; probably temperature and photochemical action are also concerned.

Assuming that the action of iodine on sulphurous acid, under the circumstances experimented on, is always represented by the equation $H_2SO_3 + H_2O + I_2 = 2HI + H_2SO_4$, it is evident that chlorine or bromine *might* cause the destruction of *more* SO_2 than corresponds to this equation, if their actions caused the formation of thionic acids: thus for tetrathionate, which, as the preceding experiments show, is only slowly acted on by iodine.

Tetrathionic Acid.

$$7\,H_2SO_3 = H_2S_4O_6 + 3\,H_2O + 3\,H_2SO_4.$$

But how it happens that a *less* quantity of SO_2 is sometimes destroyed by chlorine than by the equivalent amount of iodine is more difficult to explain: possibly, though this does not seem a very probable supposition, under the influence of light, or of other unknown circumstances, free oxygen may be formed which escapes without exerting its due influence on the sulphurous acid.

$$2\,H_2O + 2\,Cl_2 = 4\,HCl + O_2.$$

That free oxygen in aqueous solution acts comparatively slowly, at any rate in the dark, on sulphurous acid solution is shown by the

following numbers :—A quantity of saturated sulphurous acid solution was largely diluted with water, saturated with air, and kept in a well-closed opaque vessel; samples taken from time to time indicated that the oxidizing action of the dissolved oxygen lasted a considerable time, 18 hours elapsing before the action became negligible, thus—A given volume (stoppered vessel containing about 300 Ccs) required

Immediately after mixture			...	42·3	Ccs of iodine solution.
10 minutes	„	„	...	40·6	„ „
20 minutes	„	„	...	40·3	„ „
1 hour	„	„	...	40·0	„ „
2 hours	„	„	...	39·9	„ „
18 hours	„	„	...	39·3	„ „
40 hours	„	„	...	39·2	„ „

However, the following experiments indicate that when the action of chlorine or sulphurous acid takes place, without exposure to daylight, the quantity of acid destroyed is always in excess of that due to the equivalent amount of iodine. The previously described experiments were performed in the diffused light of a laboratory; but the subsequent ones were all carried out in a cellar only lighted artificially, even the weighings of the chlorine solution being performed there in order to avoid any possible photo-chemical action.

Weighed bulbs of chlorine solution were broken under known volumes of sulphurous acid, diluted the instant previously with warm or cold water, so as to obtain varying temperatures, and similar determinations were made by breaking chlorine water bulbs under potassium iodide solution at varying temperatures.

From 12 determinations the following mean numbers were obtained:—

Iodine and Sulphurous Acid.		Chlorine and Sulphurous Acid.		Rates.
Temperature.	Iodine representing SO_2 destroyed by I.	Temperature.	Iodine representing SO_2 destroyed by Cl	
14°	104 Ccs.	16°	132 Ccs.	100 to 127
40°	105 „	35°	186 „	100 to 177

It thus appears that while variation of temperature makes but

little alteration in the action of iodine, it has a great influence on that of chlorine, the difference being greater the higher the temperature, and always indicates the destruction of more SO_2 by chlorine than by iodine.

To find if the action of chlorine and iodine becomes identical at 0° with any strength of chlorine solution, bulbs containing chlorine water of varying strengths were broken.

A. Under known volumes of sulphurous acid solution at the temperature of the cellar (viz. 12° C throughout this set of determinations).

B. Under sulphurous acid cooled to 0° by additions of fragments of pure ice (found to have no effect when melted on the iodine solution).

C. Under potassium iodine at 12° and at 0°: the figures obtained at these two temperatures never differed by more than 0·1 Cc from one another (out of 40 — 50 Ccs used).

The following mean numbers were got from 33 determinations :—

Grammes of Chlorine per litre of solution.	Iodine representing SO_2 destroyed by I (from Cl and KI).	Iodine representing SO_2 destroyed by Cl at 12° C.	Iodine representing SO_2 destroyed by Cl at 0° C.
*7·20	100·0	106·1	103·4
0·68	100·0	102·5	103·9
0·78	100·0	103·3	102·6

It thus appears that at 0° there is little difference between the action of a saturated solution of Cl and one containing only about one-ninth of that amount.

These experiments would tend to show that when the action of chlorine or sulphurous acid takes place, not under the influence of light, a larger quantity of acid is destroyed than corresponds to the equation $H_2 SO_3 + Cl_2 + H_2 O = 2 H Cl + H_2 SO_4$, this extra amount varying with the temperature, and possibly with the strength of the solution; whilst if the action takes place in daylight the result is so modified that sometines more and sometimes less acid is destroyed than that indicated by this equation. In the case of bromine also, apparently, there is some irregularity in the action under different circumstances, as is shown by the non-uniform results obtained on

repetition of determinations. Iodine, on the other hand, as Bunsen has shown (Ann. Ch. and Pharm., lxxxvi, 265), acts with great regularity and uniformity in accordance with this equation under considerably varied circumstances, unless a solution containing more than 0·4 grammes of SO_2 per litre be used.

The CHAIRMAN—We have listened with much pleasure to Mr. Wright's excellent paper. It strikes me that we shall be very much benefited hereafter by reading in print the various experiments which it records. We shall be very happy to hear any remarks that any gentleman may have to make on the paper, which appears to me to contain many facts that are new and important in many ways.

Mr. PROCTOR—Mr. Wright has given us the results of experiments performed in the light, showing discrepancies among themselves. I should like to ask if Mr. Wright has any record of the intensity of the light to which they were exposed during the process?

Mr. WRIGHT—I performed the experiments in the ordinary light of the laboratory, except in one or two instances, in which I operated with the faintest practicable light of the gas. Subsequently, when the experiments were repeated in the dark, uniform results were got with the chlorine.

Mr. SHERER—May I be allowed to refer to the paper which was read before the members of the society by Mr. Pattinson, in which the latter pointed out a probable source of error in the Fresenius and Will's method of testing manganese, and said, speaking of hard ores containing magnetic oxide of iron, "that in order to effect their decomposition it is necessary to add a large quantity of sulphuric acid, and to apply extraneous heat for a considerable time. It is probable that during this lengthened heating steam is allowed to escape with the carbonic acid, and hence a higher amount of peroxide is indicated than is present in the ore." That this could not have been so in the higher results obtained in the case of the sample used in the experiments given in the paper read before the society on the 25th of November, is evident from the fact that in the three tests by Fresenius and Will's method there given, extraneous heat was in no case applied to the apparatus, and therefore the boiling point was not reached.

I have since tested three different samples of manganese by the Bunsen and Fresenius and Will's method, which by the latter gave results from one to one and a half per cent. higher than by the former, and in testing them by the Fresenius and Will's method I took the precaution of connecting the apparatus with weighed chloride of calcium tubes during the heating and drawing of air through the apparatus, and found, after repeated heating to the boiling point, no appreciable increase in the weight of the chloride of calcium tubes, and after about the third heating no appreciable decrease in the weight of the apparatus.

The CHAIRMAN—It appears to me a very important matter to those who are practically occupied in the working of manganese ores that the results of testing by different methods are so entirely different. We have a cargo or two working at present, and not only is the produce entirely different from that of many qualities of manganese, but it seems almost impossible to get the acid to act completely upon the manganese, and therefore, from a commercial point of view, it is extremely important that we should have a test for getting the commercial value.

Mr. SHERER—With regard to the objection which has been made to the Bunsen method, on the ground of the small quantities operated upon, I am now using double the amount used in the experiments given in the paper read before the Society on the 25th of November, and have obtained equally sharp results.

Mr. WRIGHT—What form of the Fresenius and Will apparatus do you use?

Mr. SHERER—The original two-flask apparatus.

Mr. WRIGHT—I never found, when using that form of apparatus, any loss of weight from escape of steam. I think the reason for this is the long extent of tube connecting the two flasks, which is always wet with sulphuric acid.

On the motion of the CHAIRMAN, seconded by Mr. PROCTOR, a vote of thanks was passed to Mr. Wright, for his valuable papers.

Mr. WRIGHT briefly acknowledged the compliment, and proposed a vote of thanks to the Chairman, which was passed by acclamation.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, MARCH 24, 1870.

MR. J. GLOVER IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Mr. John Mitchell, Monkton, was elected a member.

The name of Mr. John G. Richardson, Springwell, was read for the first time.

Mr. FREIRE-MARRECO read

NOTE ON THE MANUFACTURE OF AMMONIUM SULPHATE.

THE point to which I desire to call the attention of the Society, is the effect of using arsenical sulphuric acid in the manufacture of this salt from gas liquor. As will be seen by the specimens on the table, a very inferior sample of acid may yield a tolerable looking salt, if it contain no great quantity of arsenic; but the very much better looking sample, marked B, which the reactions exhibited show to be loaded with this impurity, besides causing great inconvenience in work, from the extra skimming required, yields a salt which is coloured bright yellow by orpiment, and very much lowered in price thereby.

Dr. LUNGE remarked, that some sulphuric acid contained very little arsenic, and if he was not mistaken, Mr. Clapham had once informed him that he had been using Westphalian pyrites which were nearly free of arsenic, and the sulphuric acid produced nearly innocuous.

Mr. R. C. CLAPHAM said, that in working several chambers together, it was found that nearly all the arsenic was deposited in the acid in the two first chambers, and the last four chambers contained very little arsenic.

Mr. FREIRE-MARRECO—This is the sample of acid I mean, which contains a large quantity of arsenic.

The CHAIRMAN—That appears to contain an exceptional quantity.

Dr. LUNGE said, that in France they had a small receiving chamber, which served principally for depositing arsenic, &c. Unless they started with a pyrites nearly free from arsenic it would be very difficult to have sulphuric acid free of it.

The CHAIRMAN inquired of Mr. Marreco if he had anything to suggest to avoid the effect of arsenic?

Mr. FREIRE-MARRECO said, his object was simply to make known the facts.

Mr. B. S. PROCTOR said, there was another point, and that was, how much arsenic would be tolerated in the acid?*

Mr. DOUGLAS (Gas Works) stated, that its presence affected the value of the ammonium sulphate to the extent of 10s. to 15s. per ton.

Mr. R. C. CLAPHAM then read the following paper.

ON A NEW METHOD OF MANUFACTURING CAUSTIC SODA, PATENTED BY M. BACHET, OF PARIS.

EXPERIMENTS were made by Lord Dundonald as far back as 1790-1794, in making caustic soda by the decomposition of common salt with litharge. This process was conducted on a manufacturing scale by the late Mr. Wm. Losh, in 1799, and I am indebted to him for some details of the process then in operation.

The proportions recommended to produce the best results were 100 parts of salt and 300 parts of oxide of lead; but when the greatest care was taken, they did not succeed in obtaining more than 3·0 parts

* The quality of the acid for this use can be judged of perfectly well by its reaction with H_2S.

of caustic soda, or 5·6 per cent. of salt was decomposed. At that time they had many difficulties to contend with, in preventing the loss of lead in the solutions, and they had no plan in operation to re-convert the white lead pigment which was formed into an oxide of lead ready to receive a fresh charge of salt. They had, therefore, to contend with considerable expenses, and some loss of lead in melting it down again to a metallic state. It was only the high price of soda, which may be calculated as equal to £85 to £90 per ton of 70 per cent. caustic soda, which enabled the operations to be conducted at a profit. I shall not trouble the members with any details of the changes which have taken place in the manufacture of soda from that time to the present. It is sufficient to say, that after three-quarters of a century has passed away, a patent has been taken out by M. Bachet, of Paris, for the production of caustic soda by means of salt and litharge, which, in some respects, resembles the old method, but with this marked difference, that hydrate of lime is added to the mixture, which has the effect of preventing the reactions which take place between the soda and the chloride of lead in the old method, resulting in the re-conversion of the caustic soda into common salt; and the new process also includes a method of regenerating the white lead pigment, so that it may be used over and over again. The experiments I am about to describe have been conducted by the Walker Alkali Co., at Walker, during the past nine months. Many trials were made as to the best mixtures to be used, but as far as has yet been ascertained the following produce good results:—

100 parts of litharge,
70 ,, salt,
50 ,, lime.

About 5 cwts. of this mixture is ground on a mill, a small quantity of water being added to dissolve the salt. It forms a white pulpy mass, which is ready to be drawn off in a quarter of an hour; indeed, as far as experiment can prove, it appears as if the decomposition was almost instantaneous, and if the operation is properly conducted, about 19 to 20 per cent. of the salt is converted into caustic soda. It will be seen hereafter how the remainder of the salt is treated. The decompositions on the mill appear to be

threefold:—the production of caustic soda, of chloride of lead, and of hydrate of oxide of lead, a portion of the salt and the litharge remaining unacted upon. The pulpy material drawn from the mill is pumped into a press, which is worked at a pressure of 145 lbs. to the square inch. A clear liquid runs out, consisting of caustic soda and brine, containing a variable quantity of lead. With the object of separating completely the small quantity of lead from the soda solutions, various plans have been tried.

Sodium sulphide separates the lead easily, but there are some objections to this process.

Carbonate of soda also precipitates it as a carbonate, but in some cases for reasons difficult to explain, the action is not certain.

The simplest method which has been pursued, is to pass the solution through hydrate of lime in the form of a filter, all the lead is precipitated, and on testing the filtered solutions, they only give a slight colour when treated with sodium sulphide.

The lime containing the lead is afterwards used on the mill in the place of fresh lime.

When the liquors are free of lead they are then evaporated in an iron pan, like an ordinary salt pan, and the salt which deposits at the bottom is gradually fished out, and may be again used with fresh oxide of lead on the mill.

But the process may be varied, so as to produce a much larger decomposition of the salt, by running the liquors from the press back again on to the mill and adding thereto fresh charges of litharge and lime.

In this way 47 to 50 per cent. of the salt may be decomposed, and converted into caustic soda, and a much stronger soda liquor is obtained to evaporate in the pans, a considerable amount of labour and cost of coals being saved. In either case the solutions are concentrated and otherwise treated till they are ready to be run into a caustic pot, where they are finished in the usual way, producing, with ordinary care, a caustic soda of 70 per cent.

We have now finished with the solutions and must return to the dry white lead cakes left in the press before described, the treatment of which forms one of the most important features in the patent.

When freed from its soda solution it consists of

Chloride of lead.
Hydrate ,,
Litharge unacted upon.
Lime ,,

As hydrate of lead has no action in decomposing salt it becomes not only a question of how to decompose the chloride of lead, but also how to convert the hydrate into an oxide which will again act upon salt. The method which appears most likely to effect these ends, is, in the first instance, to dry the material from the press, at a temperature of about 350° Fah. It turns to a fine orange yellow colour, and the hydrate is decomposed and is converted into a heavy oxide.

The mass is then slowly thrown into boiling lime water, by which the chloride of lead is decomposed, forming chloride of calcium and oxide of lead. The solution is now run off, and after one or two washings in fresh lime water, the oxide of lead is again fit to be used at the mill. The liquors which are run off however, are not to be thrown away, as they contain lead in solution, and it is necessary to separate it to prevent loss.

This is not a difficult operation, as in the cooling of the hot liquors the lead nearly all deposits in fine long crystals, and the remainder left in the solution is precipitated by means of common salt or hydrochloric acid, so that a mere trace remains on testing with sodium sulphide; the chloride of lead thus obtained is decomposed with lime water as just described, and the oxide applied to the mill. From this it appears that with ordinary care, all the lead may be recovered, but it is only fair to allow that in conducting a large manufactory, a portion will be lost, which requires to be estimated for in the cost of production.

Thus far the process appears to work quite smoothly, but the regenerated lead obtained as above described, does not in all cases act the same. Sometimes its action is quite as energetic on salt as the original litharge, but in other cases it has proved less satisfactory when worked frequently over again in the process. The cause of this change is not always sufficiently clear; in some instances, during the heating of the material, $Pb_3 O_4$ and $Pb O_2$, are formed, both of

which, I find, have no action on salt. Another difficulty has been in the rapid absorption of carbonic acid by the hydrates both of lime and lead, resulting in a product which has not only no action on salt but appears, to a large extent, to prevent the free action of the oxide of lead. When, however, the process is conducted quickly, the regenerated oxide of lead may be used over and over again more frequently; but, so far as experiment has proved, it appears likely that after five or six operations, it will be necessary to calcine a portion of it, and this is proposed to be effected as under. It is mixed with water, and the lighter portion syphoned off, settled and dried. This portion, which is found to contain nearly all the carbonates, is then calcined at a strong red heat, in a furnace constructed with a working bed of hard mountain limestone, but a roof of ordinary fire bricks. It parts with its carbonic acid quite easily at a red heat, and is then ready to be used in the mill again.

Such is briefly an outline of this patent, and the importance of a new process, with the object of producing caustic soda by more direct means than at present exists, cannot be exaggerated. Without taking into account the total production of soda, but confining the calculation to caustic soda alone, we find that 20,000 tons of this article are now made in England, of the value of £350,000 annually. The plant required is not expensive, consisting chiefly of mills, presses, evaporating pans, and furnaces.

On the whole there appears a reasonable prospect, when M. Bachet's patent is fully worked out in practice, that caustic soda will be made at a considerable reduction in cost on the present plan. But as there are gentlemen present who understand these subjects in all their bearings so well, I now leave the result of the experiments in their hands, and shall be happy to hear their criticisms.

The CHAIRMAN said, that some years ago, he had tried experiments by simply dropping slowly chloride of lead into cold lime water, and found that chloride of calcium and hydrate of lead were formed, but that boiling lime water should give them an oxide of lead.

Mr. CLAPHAM said, that hydrate of lead was decomposed in boiling water and converted into a heavy crystalline oxide, but it was

found best to heat the material first, as described in the paper, and then add it to the hot lime water.

Mr. HILL (Jarrow Chemical Works) said, that it had been ascertained by experiment that soda manufacturers could not easily separate common salt from caustic liquor, and that in his opinion 70 per cent. could not be made.

Mr. CLAPHAM said, that several plans had been tried to get over the difficulties referred to by Mr. Hill. They could free the solutions from salt to a very large extent, but still the salt did carry away with it portions of caustic soda, but these were got back again when it was applied to the mill, so that none was lost; but there were several matters with regard to the better separation of the caustic soda from the salt which required further attention.

Mr. W. CAIL said, that he had frequently obtained 72 to 73 per cent. of caustic soda.

Mr. CLAPHAM said, their experience was as yet very limited, and the manufacture required a great deal of careful attention, and that they would be very happy to receive the assistance of the members.

The CHAIRMAN—Have you satisfied yourself as to the loss of lead in the process?

Mr. CLAPHAM said, that a great deal of attention had been paid from the first to that matter, and he thought that all the lead could be got out of the solutions.

The CHAIRMAN—You will always have a quantity of peroxide of lead present?

Mr. CLAPHAM—We expect by heating to drive off the carbonic acid, and at the same time to reduce the peroxide to oxide, but this so far is imperfectly worked out.

Dr. LUNGE asked whether peroxide of lead was generally obtained. If it could be obtained at a cheap rate it would be very valuable in glass making.

Mr. HILL said, most manufacturers regarded the process as a most expensive one.

Mr. PROCTOR—What was the objection to it?

The CHAIRMAN thought it need not be expensive.

Mr. HILL said, that carrying the process out to nearly a finished

state, and then carrying it back again to begin operations afresh would be very expensive. There was the difficulty of separating the caustic soda from the salt which had to be fished up, and this had not hitherto been accomplished by manufacturers.

The CHAIRMAN said, that had reference to the old process; but they had not that difficulty here, because they had no sulphate, only common salt.

Mr. HILL said, his objection held good in a higher degree with common salt than with sulphate.

The CHAIRMAN said, the results of the experiments were very valuable, and he trusted the subject would be one for further investigation by the members.

Dr. LUNGE then read a paper "On the Analysis of Refined Lead," by Dr. Fresenius (Translation).

ON THE ANALYSIS OF REFINED LEAD.

THE improvements in the metallurgy of lead, especially by Pattinson's process and the new desilverizing process by means of zinc, have made such progress that the refined lead of many smelters contains from 99·94 to 99·995 per cent. of lead. Although this would, perhaps, appear almost tantamount to chemical purity, still the buyers of refined lead make great distinctions between the different brands, as even extremely small traces of iron, copper, &c., exert a notable influence when lead is employed for the manufacture of flint glass, white lead, &c. The consequence is that refined lead is very often the object of chemical analysis, and special precautions must be taken to estimate very small quantities of foreign metals in nearly pure lead.

The foreign metals which are here to be considered are—silver, copper, bismuth, cadmium, zinc, iron, nickel, antimony, besides cobalt, manganese, and arsenic, but these three last are rarely present in an estimable quantity. The foreign metals come partly from the lead ore, partly from the zinc, if this has been used for desilverizing.

Even if 200 grammes of lead are used for each analysis, still the separated compounds of the foreign metals are so insignificant, that

it was necessary in the first place to examine the degree of accuracy that could be reached by the methods of separating them. For this object many preliminary experiments were made, and their results are as follows :—

IRON.—Three volumetric methods and the precipitation by ammonia and weighing were tried. Both the permanganate of potassium and the chloride of tin give more accurate results then the gravimetric method; the estimation by iodide of potassium and hyposulphite of sodium is less accurate, *e.g.*, the permanganate showed 2·105 milligrammes instead of 2·073, and 3·692 of calculated instead of 3·628.

COPPER.—Estimated gravimetrically as protosulphide and as oxides, and volumetrically by iodide of potassium and hyposulphite of sodium. The latter is far more accurate than the former; the results were:—calculated, 1·737 milligrammes; found, 1·763; calculated, 2·672 milligrammes; found, 2·739.

ZINC.—Estimated gravimetrically, first, by precipitation with carbonate of sodium; second, in the same way, but drenching the filter, previous to igniting, with a solution of ammonium nitrate; third, as sulphuret—and volumetrically (1) by potassium ferricyanide and potassium iodide, (2) by persulphate of iron and permanganate of potassium. Both the latter methods were far more accurate than the gravimetric methods, especially the ferricyanide; it showed 1·973 resp. 2·850 milligrammes, instead of 1·935 and 2·838.

ANTIMONY.—Estimated, first, by precipitating as sulphuret and drying on the filter; second, by precipitating as $Sb_2 S_3$, filtering through asbestos, and igniting in a current of carbonic anhydride; third, volumetrically by iodine; fourth, the same, by protosulphate of iron and bichromate of potassium. The first method is the least accurate, the second and third are about equal, the fourth is the best. It showed 1·831 and 3·527 milligrammes instead of 2·007 and 3·513.

ARSENIC, BISMUTH, CADMIUM, AND SILVER were not examined separately, because the attainable degree of accuracy in their estimation is partly well known, and partly quite analogous to those mentioned above. The general conclusion is, that in estimating quantities of a few milligrammes of metals, good volumetric methods give errors of 0·05—0·2 milligrammes, and good gravimetric methods, errors

of 0·2—0·3 milligrammes. When the precipitates have to be weighed on dried filters, errors of 0·3—0·7 milligrammes cannot be avoided; consequently, when many estimations have to be made, the volumetric methods are preferable on every account, but for a single analysis, it does not seem worth the while to prepare the necessary test liquors, and the gravimetric methods seem quite accurate enough. In every case it is necessary to employ as large quantities of material as is practicable for analysis.

METHOD OF ANALYSIS.

1.—The lead is cut into large pieces, and the surface of each of them pared completely clean with a bright knife-blade.

2.—Two portions of exactly 200 grammes each of these clean pieces are accurately weighed off, and each portion is dissolved in a large flask of 1 to 1½ litres capacity, in pure dilute nitric acid. The quantity required is, 550 CC of 1·2 spec. grav. and the necessary quantity of water to prevent any separation of lead nitrate. The solution is assisted by gently heating; an excess of acid is to be avoided. The liquid is left standing for 12 to 24 hours. If there is no great excess of acid, 1 litre of liquid will hold all the lead nitrate formed (310 grammes) in solution.

3.—In most cases the solutions from refined lead are perfectly clear; only in rare cases, in the presence of somewhat more antimony, a precipitate is formed at this stage. This case is treated further on (§ 17). At present, only the *clear* solutions are spoken of. One of them (A) serves for the estimation of silver, the other (B) for all the other metals.

4.—The solution A is diluted to 1500 CC, and 1 CC hydrochloric acid of 1·12 spec. grav., mixed with 50 CC water, is added. This quantity is more than sufficient to precipitate all the silver, but much too small to precipitate any lead chloride. The liquid is left standing for 2 or 3 days; if a precipitate is formed, the clear part is siphoned off, the precipitate is collected on a small filter, washed with boiling water, dried, ignited in a small porcelain crucible, in a current of hydrogen, and the metallic silver weighed as such. To control the

result, the silver is re-dissolved in nitric acid and estimated volumetrically by the solution of iodine and starch. (Pisani's method.)

5.—The solution B is transferred to a marked flask of 2 litres' capacity; about 115 grammes (say 62 or 63 CC) of pure concentrated sulphuric acid are added, the mixture left to cool, and then filled up to the mark, well shaken, and left to subside. The quantity of sulphuric acid is so calculated that there is about 10 or 12 grammes in excess. The liquid, when completely or nearly clear, is drawn off by a siphon filled with part of it (this is preferable to filtering); in this way more than 1750 CC can be obtained, but 1750 CC are exactly measured off and evaporated in a completely clean acid bath, till copious fumes of sulphuric acid appear and show that the nitric acid is driven off. After cooling, 60 CC water are added, and the small quantity of lead sulphate is separated by filtering through a small filter (previously purified by washing with hydrochloric acid and water), and well washed.

6.—This small precipitate of lead sulphates often contains small quantities of antimony. It is dissolved in the least quantity of hydrochloric acid, diluted with ten times its bulk of solution of hydrosulphuric acid, and treated with a current of hydrosulphuric acid gas. The new precipitate is filtered and washed, the filter spread out on a porcelain dish, and digested at a nearly boiling heat for a short time, with a solution of pure potassium or ammonium sulphide and a small quantity of pure sulphur, the mixture is filtered, the filtrate acidulated with hydrochloric acid, and the new precipitate left to settle in a gentle heat.

7.—The solution obtained by sulphuric acid in § 5, is diluted to 200 CC, heated to 70° C, and at this temperature H_2S is sent through it till the precipitate settles, then it is left to settle for 12 hours at a moderate heat, filtered through a small filter and washed. The filtrate and washings are treated according to § 12, but the small precipitate is treated with K_2S solution and pure sulphur, just as in § 6; the K_2S filtrate is acidulated with H Cl, and left to settle.

8.—The residue not dissolved in K_2S is spread with the filter in a small porcelain capsule, and digested almost at a boiling heat with dilute nitric acid (say 1 part of acid of 1·2 sp. gr., and 2 parts of

water). After solution the liquid is filtered, the filter washed and ignited, and the ash added to the nitric acid solution. The whole is now evaporated, after adding 2 CC dilute sulphuric acid, till all the nitric acid is driven off, some water is added, the trace of lead sulphate which may have been formed is filtered off, the liquid nearly neutralized with pure potassa, some sodium carbonate and potassium cyanide (free from sulphide) added, and gently heated. If a precipitate forms, it is washed, dissolved in dilute nitric acid, and the bismuth precipitated by ammonium carbonate, and weighed as oxide. To the solution, freed from bismuth, a little more potassium cyanide and a little sulphide are added. If a precipitate falls it may contain cadmium and silver sulphides. It is filtered off, dissolved in dilute hot nitric acid, the silver precipitated with a few drops of H Cl, the filtrate evaporated nearly to dryness, and examined whether any cadmium can be precipitated by sodium carbonate, if so, it is weighed as oxide, moistening the filter with a little ammonium nitrate, to obviate the reduction and loss of cadmium. The liquid free from Cd and Ag is evaporated with a little sulphuric, nitric, and hydrochloric acid, till the smell of hydrocyanic acid is quite gone, the clear (or filtered from Ag Cl) solution is precipitated with H_2S, and the copper weighed as protosulphide. If its quantity is very small, the estimation is controlled by re-dissolving in nitric acid, evaporating with sulphuric acid, and decomposing with potassium iodide.

9.—The precipitates obtained in § 6 and 7 from the K_2S solutions by acidulating, are filtered off on a small filter, washed, dried, repeatedly washed with carbon bisulphide, to remove any free sulphur, and dried again. Then the filter and precipitate are put in a small porcelain capsule, covered over with a water glass, and red fuming nitric acid is added; the whole is gently heated till the paper is quite destroyed, and most of the acid driven off. Then a little water is added, sodium carbonate in excess, and sodium nitrate. The mass is evaporated to dryness, and cautiously heated till it melts and becomes white. The cooled cake is triturated in a mortar with a little cold water, and then washed first with a little water, then with a mixture of water and alcohol. The remaining sodium antimoniate is dissolved in H Cl and a little tartaric acid, precipitated with H_2S, and allowed to settle.

10.—The aqueous solution of the melted mass obtained in § 9, which contains the As and a little Sb, is freed from alcohol by evaporation, acidulated with dilute sulphuric acid, evaporated till all nitric acid is driven off and precipitated at 70° C with H_2S. The precipitate is filtered and washed and heated on the filter with a cold concentrated solution of ammonium carbonate, pouring the filtrate back again repeatedly. A little Sb_2S_3 and S remain on the filter; it is dissolved by strong H Cl, precipitated with H_2S and the Sb estimated along with the larger quantity obtained in § 9. The best method for it is filtering through asbestos, in a drawn-out tube, and freeing the Sb_2S_3 from water and free sulphur by heating it in a current of CO_2. Very small quantities may be controlled volumetrically.

11.—The solution of As_2S_3 in ammonium carbonate (§ 10) is acidulated with H Cl, some more H_2S conducted through it, and if a weighable quantity of As_2S_3 should be precipitated, it is filtered off in an asbestos tube, dried at 100° C, and weighed.

12.—The filtrate and washings obtained in § 7, if they measure more than 500 CC, are evaporated, just made alkaline by ammonia in a flask, and ammonium sulphide added. The flask is filled up to the neck, stoppered, and left standing for 24 hours. It must not be filtered till the small precipitate has entirely subsided. The filtrate is just acidulated with acetic acid, and evaporated at a gentle heat, in order that any nickel sulphide dissolved in the ammonium sulphide may be separated along with sulphur. This is filtered off after subsiding.

13.—The ammonium sulphide precipitate filtered in 12 is treated on the small filter itself immediately after filtering with a mixture of about six parts H_2S water and one part hydrochloric acid of 1·12 spec. grav., repeatedly pouring back the liquid. In this manner it is possible to dissolve zinc and iron sulphides, and to leave cobalt and nickel sulphides on the filter. The latter is ignited together with that obtained in § 12, which may contain nickel along with sulphur, then treated with a little aqua regia, evaporated almost to dryness, just made alkaline with ammonia, and a little ammonium carbonate, filtered, and the filtrate heated with pure potassa solution in a platinum dish till all ammonia is removed. If appreciable flakes are separated they

are filtered, dried, ignited, weighed, and the nickel protoxide examined by the blowpipe for cobalt.

14.—The very dilute H Cl solution got in § 13 is concentrated with addition of nitric acid, precipitated with ammonia, and heated; the hydrated iron peroxide is filtered off, dissolved in H Cl, precipitated with NH_3, washed, dried, ignited and weighed. To control it, it is fused with potassium disulphate, reduced by zinc and tested with permanganate.

15.—The filtrate from the iron peroxide is digested with ammonium sulphide, at least for 24 hours. Any weighable flakes are filtered off, washed, and treated on the filter with weak acetic acid, to remove manganese sulphide. If the residue is quite white, it may be weighed as zinc sulphide after drying, and the estimation may be checked volumetrically. The acetic acid solution is concentrated and examined by potassa for manganese.

16.—Previous to calculating the results, it must be considered what quantity of lead corresponds to the 1750 CC employed. For this is necessary to know what space the lead sulphate from 200 gr. occupies in the measuring flask. At 16° C, this is 45 CC; the 2000 CC, therefore, contained 1955 CC solution and 45 CC lead sulphate, and the 1750 CC employed correspond to 179·03 grammes lead. All foreign metals, except the silver, have, therefore, to be calculated for this.

17.—Lastly, a case must be mentioned which happens with lead rich in antimony. Then, in preparing the solution a residue is left, containing oxides of antimony, and sometimes also arsenic. Two plans can be followed—either the precipitate can be analysed separately; of course, it must be calculated for 200 gr. lead, whilst the metals from the solution only refer to 179 gr.; or, the precipitate is filtered off, dissolved in H Cl, and precipitated by H_2S at 70° C. The nitric acid solution is precipitated by sulphuric acid, the lead sulphate filtered and washed with weak sulphuric acid; the filtrate and washings are concentrated and analysed as above; the precipitate by H_2S from the sulphuric acid solution is treated along with the one received above.

The latter process has the drawback that the lead sulphate must

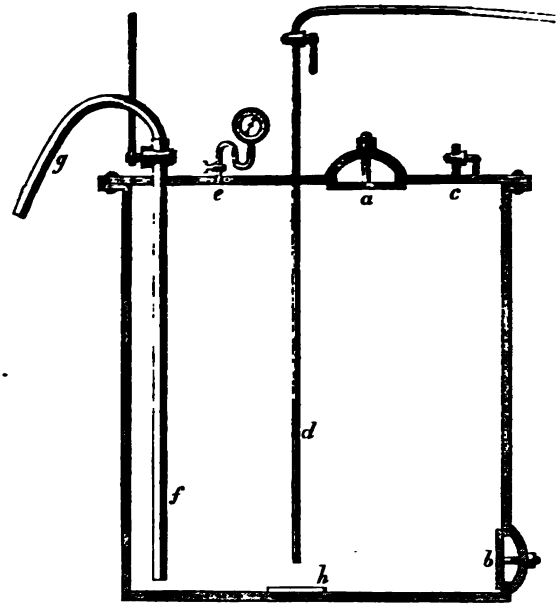
be washed, but the advantage that arsenic and antimony need not be estimated in two places. All metals are calculated for 200 gr. lead.

18.—Any other metals not mentioned above, must, of course, if they be present, influence the process.

19.—Lastly, the quantity of Pb is calculated from the difference. A direct estimation would not give any control over the correctness of the estimation of the foreign metals.

On the motion of the CHAIRMAN, votes of thanks were passed to the authors of papers.

APPARATUS FOR MELTING SULPHUR BY HIGH-PRESSURE STEAM, USED BY THE TYNE CHEMICAL COMPANY, SOUTH SHIELDS.*



a manhole for filling.

b manhole for emptying residues.

c air tap.

d steam-pipe (1 inch bore).

e pressure-guage.

f blow-off pipe (2 inches bore) for melted brimstone, water, and dirt; the cock of it to be immediately outside the cylinder, with a long handle and a moveable bend *g*.

h plate below the steam-pipe, to prevent the bottom of the vessel from being worn out by the impinging steam.

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* See page 130.

Newcastle-upon-Tyne Chemical Society.

ANNUAL MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, OCTOBER 27TH, 1870.

I. LOWTHIAN BELL, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the last meeting were read and confirmed.

The SECRETARY read the report of the Committee, with the statement of the Treasurer, as follows:—

Dr.

	£	s.	d.
To 14 Subscriptions for 1868–9	7	0	0
„ 79 „ 1869–70	39	10	0
	£46	10	0
Balance in hand	£7	15	4
8 unpaid Subscriptions due for 1868–9.			
18 „ „ 1869–70.			

Cr.

	£	s.	d.
By Balance from 1868–9	3	12	11
„ Advertising in Newspapers	0	6	4
„ Reporter, J. Hornsby	6	6	0
„ Cost of Printing Transactions for 1869–70.—A. Reid	18	14	0
„ General Printing, Stationery and Postage.—A. Reid	5	2	11
„ Subscriptions for "Annales des Mines" and "Zeitschrift für Analyt: Chemie"	1	11	0
„ Postage and Stationery	1	9	6
„ Commission for collecting Subscriptions	1	12	0
„ Balance	7	15	4
	£46	10	0

JOHN PATTINSON.

Compared with vouchers and found correct,

BARNARD S. PROCTOR.

Newcastle-on-Tyne, Oct. 20th, 1870.

COMMITTEE'S REPORT.

Your Committee are glad to be able to report, on the whole, a fairly satisfactory progress of the Society during its second year.

The number of members continues about the same, the fresh elections being nearly balanced by resignations, which latter are without exception those of members leaving the neighbourhood.

The Treasurer's statement shows a fair balance in hand in spite of a too large number of unpaid subscriptions. It may be observed that the new postal regulations will very materially diminish our expenses for postages; and at the same time, it is expected, materially facilitate our operations.

The papers read at the ordinary meetings have all borne more or less directly on Technical Chemistry, and have in so far been of a nature eminently appropriate to this district. Your Committee would urge on members the importance of exerting themselves to contribute to our meetings observations, no matter how short, on unrecorded facts which come under their notice.

The experiment of publishing the Transactions of the Society in monthly parts, resolved on at the last annual meeting, has been tried. It now rests with this meeting to decide whether it shall be persevered with.

In accordance with a suggestion from some of our number, refreshments will in future be provided at the close of the ordinary meetings free of charge to members.

Mr. R. C. CLAPHAM—I beg to propose that the Secretary and Treasurer's report, which must be highly satisfactory to the meeting, be adopted.

Mr. B. S. PROCTOR seconded the motion, which was carried unanimously.

ALTERATION OF RULES.

Mr. B. S. PROCTOR rose to move the resolution of which he had given notice. He said—I have given notice of this motion as much on account of the Committee in general as on my own. At one of our recent meetings we found that Rule 6 would not work quite satisfactorily; it required some little alteration to make it less ambiguous and more practical. Rule 6, as it at present stands, is as follows:—

The Officers shall be elected at the annual meeting, and shall consist of President, Treasurer, two Secretaries, and a Committee of ten. Any member may nominate any eligible person or persons (not exceeding the required number) to fill up the respective offices, and voting papers shall be supplied

to members ten days previous. to the annual general meeting, upon which members shall write the names of the members they propose as officers, and deliver the same to the Secretaries, or at the meeting. The Chairman shall appoint scrutineers, who shall examine the lists.

We found that rule left us without any very evident manner in which the members were to nominate those they desired to be elected to the Committee, and the result was probably that no new members would be nominated, or that the members of the Society generally would not nominate, and that the nominations would be few, and not of a satisfactory description. Also, that the President, should he be elected at the annual meeting, would, perhaps, come into office with the members of the Society expecting him to give an inaugural address, and without due notice to prepare that address. And several other little defects seemed to exist in the rule. The alterations which I propose, and which I may say represent the views of the Committee, I have divided into sections, which will admit of having each part separately discussed. I will just read the alterations as a whole first, and then we can discuss the sections one by one. After reading the resolution Mr. Proctor moved the first section:—

The officers shall be nominated at the penultimate and elected at the last meeting of the session; they shall consist of President, Vice-Presidents, Treasurer, two Secretaries, and a committee of ten.

Mr. R. C. CLAPHAM seconded the motion.

The CHAIRMAN took a show of hands, and declared the first section carried unanimously.

Mr. PROCTOR—I move the second section:—

Any member may nominate, in writing, to the Secretaries, any eligible person or persons (not exceeding the required number) to fill up the respective offices.

Voting papers, containing all the nominations, shall be supplied to members ten days previous to the day of election, from which members may erase the names of those for whom they do not vote, and return the paper to the Secretaries at the meeting for election.

That is almost as it stood before, except the statement that they shall be nominated in writing to the Secretaries.

Mr. BERKLEY seconded the motion.

Mr. J. W. SWAN—I move, as an amendment, the addition of the words "or send it by post to the Secretaries, with the signature of the member."

Mr. ALLHUSEN—I second Mr. Swan's amendment.

Mr. PROCTOR—I agree to it.

The clause was then carried as modified by Mr. Swan's amendment.

Mr. PROCTOR moved the next clause:—

The Chairman shall appoint Scrutineers who shall examine the voting lists, and an Auditor who shall verify the Treasurer's accounts.

The PRESIDENT—I would suggest that there be two auditors; that is the usual way.

Mr. PATTINSON—The only objection is the one Mr. Proctor alluded to: the difficulty of securing two auditors.

The PRESIDENT—It appears to me that it would be easy, as the work would have to be done at the meeting.

Mr. CLAPHAM—The wording would have to be different; the auditors would have to be appointed by the members, not by the chairman.

The PRESIDENT—Exactly.

Mr. CLAPHAM—Would it not meet the case to put the word "auditor" in your first paragraph, and then he would be appointed the same as the other officers; and then, the chairman, according to rule, at present, appoints the scrutineers?

The PRESIDENT—It is a matter of no moment who appoints the auditors.

Mr. SWAN—"Auditor" will do?

The PRESIDENT—Yes, one auditor.

Mr. PROCTOR—I move it in the amended form.

Mr. CLAPHAM—I second it.

The PRESIDENT—We will take these two alterations together.

Agreed to.

Mr. PROCTOR moved the next clause:—

The President shall not hold office for more than two consecutive years, and the retired Presidents shall constitute the Vice-Presidents of the Society.

Mr. CLAPHAM—I beg to second that.

The PRESIDENT—I am entirely in favour of that. It would be a great mistake on the part of the Society if it allowed any gentleman to hold office as it were during his lifetime. It amounts to that. As Mr. Proctor has very properly said, it places the other members in rather a delicate position. They might hesitate about displacing a president, and I undertook office on the distinct understanding that that had to be the rule. I mentioned it at the time to Mr. Marreco, and I have much pleasure in putting it now.

The motion was then put and carried.

Mr. PROCTOR moved the last clause:—

A President may be re-elected after the lapse of four years.

Mr. CLAPHAM—I think that may be made "two."

Mr. GREGSON—It is much better, you might lose a good man and be anxious to get him back again.

Mr. PROCTOR—I accept the alteration.

The motion as altered was then put and carried.

ELECTION OF MEMBER.

Mr. John Geo. Richardson, Jarrow Paper Mills, was unanimously elected a member.

APPOINTMENT OF SCRUTINEERS.

The PRESIDENT appointed Mr. Reid and Mr. Allhusen as scrutineers of the voting papers.

PROPOSED VISITS TO NEW WORKS.

Mr. R. C. CLAPHAM—I would make a suggestion connected with the Society which may possibly be useful. It has occurred to me that the members might occasionally meet together and devote an afternoon to examining the new processes that are going on on the Tyne. Within the last few years, for instance, there has been a very important branch of business established, the manufacture of copper; and

there are always new inventions going on, which are exceedingly interesting to the members. It is desirable that we should occasionally have the privilege of seeing these new things, and it strikes me that on such occasions, instead of always devoting our time to papers, we might occasionally vary it by such an arrangement, and that afterwards, the members from that locality might dine together. I think it would create a little social and pleasant feeling which the Society seems rather to want.

After some discussion the question was referred to the committee.

The Scrutineers reported that Mr. John Glover was elected President; Dr. C. R. A. Wright to serve on the Committee in his stead; and the remaining officers were re-elected.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, NOVEMBER 24TH, 1870.

JOHN GLOVER, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

The following names were read for the first time: Mr. Walter Bowman, Gateshead; Mr. W. M. Sowerby, Jarrow.

Dr. LUNGE read,

NOTES ON THE PROGRESS OF FOREIGN ANALYSIS.

(*From Fresenius' Zeitschrift.*)

Mohr (Vol. VIII., p. 13) proposes to protect solutions of bichloride of tin, sulphuretted hydrogen, caustic baryta, sulphurous acid, and the like, against the action of atmospheric oxygen and carbonic acid, by covering them with a layer of petroleum.

Reichardt (p. 116) recovers uranium oxide from the residues accumulating from estimations of phosphoric acid by dissolving the uranium phosphate in hydrochloric or nitric acid, adding an excess of ferric chloride and sodium acetate, diluting and boiling for some time. The phosphoric acid is completely precipitated along with basic acetate of iron, and all uranium remains in solution. A cheaper method is to precipitate the acid solution of uranium phosphate after adding an excess of ferric chloride with sodium carbonate. The excess of the precipitant retains the uranium oxide completely in solution; the filtered solution is acidified with hydrochloric acid, heated

till all carbonic acid is driven off, and the uranium oxide precipitated by ammonia.

Kubel (p. 125) draws attention to the fact that the precipitate caused by ammoniacal solution of magnesia in the solution of a phosphate contains more magnesia than corresponds to the calculated formula, and that this precipitate must therefore be redissolved and reprecipitated to obtain accurate results.

Cossa (p. 145) gives the results of careful experiments upon the solubility of calcium carbonate, dolomite, etc., in water saturated with carbonic acid.

P. 174, Bunsen's paper on his filter pump.

Gerlach (p. 245 to 298) gives a number of exceedingly valuable tables of solubility, embracing, perhaps, all published determinations of this character in a synoptic form. This part can be purchased in a special reprint.

Trommsdorff (p. 330 to 370) gives a complete course of instructions for water analysis, which also deserves to be published separately. There are copious additions to it in Vol. IX., p. 157 to 176.

Schönn (p. 380) describes a method of decomposing many refractory substances by means of sodium or potassium, in a steel crucible.

Emmerling (p. 434) describes the action of boiling liquids, water, acids, alkalies, &c., on glass and porcelain vessels. As Fresenius has done before, he finds that hydrochloric and nitric acid act on glass less than boiling water, but the addition of sulphuric acid, and especially of caustic or carbonated alkalies acts in the opposite way. He, therefore, strongly advises to avoid evaporating solutions ever so slightly alkaline, not to use quite new vessels, which are far more acted upon than when they have been sometime in use, and to diminish the bulk of the washings, which can be easily done by the use of Bunsen's filter pump. Especially he recommends the use of porcelain vessels in preference to glass, wherever practicable, as water and acids have next to no action upon them, and alkalies much less than upon glass.

Böttger (p. 449) recommends test paper stained with an alcoholic

solution of alkanine. The smallest trace of alkalies, especially of ammonia, changes its deep red colour into blue.

Stein (p. 450) describes the way to prepare ultramarine paper, for the purpose of testing for free acid in commercial sulphate of alumina, and other salts, which even in the completely neutral state act upon the ordinary test papers like free acids, but not upon ultramarine, which is only discoloured by really free acid.

Poleck (p. 451) asserts that the formation of carbonic oxide, which has been observed during the action of potassium pyrogallate on oxygen, or mixtures containing it, and which has been believed to render this analytical method useless, takes place only when the proportion of oxygen in the mixture is greater than in atmospheric air. In the latter, or in mixtures containing less oxygen, no carbonic oxide at all is formed by this method.

Löwe (Vol. IX., p. 20) proposes to substitute for the usual test solution of alkaline copper tartrate, another alkaline solution of copper, produced by the help of glycerine. It can be made more quickly, does not incline to the formation of fungi, and is much more stable, even in diffused daylight. To a solution of 16 grammes pure crystallized copper sulphate in 64 grammes water, there is added in the cold 80 CC caustic soda solution of 1·34 specific gravity, and the mixture is shaken up with 6 to 8 grammes pure glycerine, till all is dissolved. If this solution shows the least turbidity on diluting with two-thirds of its bulk of water, or on heating, a little more caustic soda and glycerine must be added. If it is desirable to have a solution free from sulphates, freshly precipitated copper hydrate is dissolved in soda solution, with the help of glycerine.

Reinige (p. 39) describes a very interesting method for the estimation of iodine, viz.: by means of potassium permanganate. Two equivalents of the latter, and one equivalent of potassium iodide, produce one of potassium iodate, two of free potassa, and four of manganese peroxide. The decomposition is assisted by boiling the solution, and if the latter is very dilute, a little potassium carbonate is added to induce the commencement of the reaction. As bromine and chlorine are not in the least acted upon under the same circum-

stances, this reaction is, perhaps, the most convenient for the estimation of iodine. The latter must be combined with potassium, and the solution rendered nearly neutral but still slightly alkaline. It is heated till it boils gently, and a solution of 2·5 grammes potassium permanganate in 497·5 grammes water is gradually added, removing the beaker from the lamp each time for a few moments, to allow the precipitated peroxide to deposit, but heating it again to the boiling point before adding another portion of permanganate. When the solution, after the precipitate has subsided, shows a distinctly reddish tint, all potassium iodide is decomposed. The small excess of permanganate is estimated by sodium hyposulphite, and the remainder shows exactly two milligrammes of iodine for each CC of the permanganate solution used. The results are very accurate.

Fresenius (p. 52) describes a number of very important experiments concerning the influence of various acids and salts on the precipitation of barium sulphate, and the admixtures of foreign salts with the precipitate. His conclusions are:—1st.—Barium sulphate does not require 43,000 parts of water for solution as usually stated, but upwards of 400,000 parts, even in presence of hydrochloric acid which slightly raises its solubility. 2nd.—Sodium chloride, potassium chloride, and barium nitrate do not increase its solubility very perceptibly, but the alkaline nitrates do so. 3rd.—Hydrochloric acid does the same, and it must, therefore, be evaporated or partially neutralized before the precipitation. 4th.—Sodium chloride does not produce any foreign admixture in the precipitate. 5th.—Potassium chlorate *does* cause such admixture in a high degree. The precipitate can, however, be purified by digesting with hot hydrochloric acid, evaporating the same and washing with water. 6th.—Sodium, potassium, and barium nitrate cause a very impure precipitate, which cannot be purified by washing with hydrochloric acid but only by fusion with sodium carbonate and estimating the sulphates in the aqueous solution of the fused mass.

Schorer (p. 213) describes and figures an extremely simple thermostat. The principle is that the air in an empty test tube inside the air bath acts by its expansion or contraction on the mercury in one

leg of an U-tube, into the other leg of which the gas delivery tube dips. The latter has a long narrow slit which is lengthened or shortened as the mercury falls or rises, and thereby increases or decreases the quantity of gas delivered. A set screw on the top allows of adjusting the dip for any temperature required.

Löwe (p. 216) gives some excellent hints on the ultimate analysis of organic bodies.

Bunsen (p. 283) describes his method of analysing the ashes left by the combustion of vegetable or animal bodies. It is precisely the same as he has taught for many years past to those working in his laboratory.

In the discussion which ensued,

Mr. R. C. CLAPHAM said, with reference to Emmerling's paper, that the glass floats and windows in vitriol chambers were very often much corroded, and that this effect, which he attributed to hydrofluoric acid, varied very much with different kinds of glass.

The PRESIDENT had seen fluor spar in cargoes of pyrites.

Mr. W. H. RICHARDSON, with regard to Stein's test paper, said he had never found commercial sulphate of alumina which did not more or less affect ultramarine.

Dr. WRIGHT suggested, as a delicate test for free acid, a mixture obtained by precipitating Prussian blue in the ordinary way, adding slowly weak caustic potash till the colour is just discharged, and diluting.

The PRESIDENT, with regard to Fresenius' paper, drew attention to the great discrepancy between results obtained here and in London from the same samples of pyrites, and promised a communication on this point.

The SECRETARY read for Mr. Henry Bowman,

ON THE OCCURRENCE OF SUPPOSED NATIVE COPPER.

During the months of March, April, and May last, we were burning at Washington a cargo of copper pyrites from the Tharsis Mines in Spain. What appeared to be a block of pyrites broke the

stone-breaking machine, being itself broken into three pieces. On being examined it showed marks of being much crushed, and turned out to be apparently a piece of native copper. The pieces exhibited weigh 21½ lbs., but as some smaller fragments may have been lost, the original mass was probably somewhat heavier. The colour varies in different parts from copper red to almost white with a hackly fracture. The interior is full of cavities about shot size. The outer surface has no adherent matrix whatever. The specific gravity is 8·4. An analysis, for which I am indebted to Mr. Wm. Moody, showed the following composition :—

	Per Cent.
Copper	84·80
Iron	5·54
Zinc	trace.
Nickel...	·28
Cobalt...	·14
Antimony	·50
Arsenic	7·10
Sulphur	·73
Oxygen	·41
	99·50

The sample for analysis was taken by breaking pieces off at different places, for in three samples the iron varied from 4·8 to 5·4 and 8·1.

With regard to its mode of deposition, I am inclined, without hazarding a positive opinion, to think that it has been reduced "in the dry way."

I am led to this conclusion by the appearances exhibited on the surfaces of fracture, which appear quite compatible with its having been reduced from a *molten, but imperfectly fluid* mass of heterogeneous composition, and this would appear to agree with the information afforded by analysis. The appearance of its outer surface seems, also, scarcely consistent with its having been reduced by any wet action from a mass of pyrites.

I should add, that not hearing of the accident until two days after its occurrence, I have been unable to obtain a very satisfactory history.

In the discussion which ensued the specimen was generally considered as doubtfully authenticated.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, DECEMBER 22ND, 1870.

JOHN GLOVER, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Messrs. Walter Bowman, Gateshead, and W. M. Sowerby, Jarrow, were unanimously elected as members. The names of Messrs. William Crowder and Henry Langdale were read for the first time.

The PRESIDENT read the following inaugural address:—

GENTLEMEN,—As I mentioned to you at our last meeting, the paper I am about to read was prepared for delivery on that occasion, but was not then read from causes which I explained. The honour you conferred on me by my selection as your President was quite unexpected on my part, and the notice of my election being of the shortest, I shall not attempt any formal address, but ask your indulgence while I point out some lines of thought and action which I think lie fairly before us as members of a Chemical Society. The first subject, both in absolute and relative importance, is that now stirring the public mind so deeply, viz., the application of the scientific knowledge of the age to the preservation of the public health generally, and its special application to the prevention of the serious mischief and waste consequent on our sewage arrangements. Many of you will be aware that this subject has been approached from almost every conceivable point, and yet, partly from its inherent difficulties, and perhaps still more from the financial difficulty and the force of habit, we appear to be as far from a true and practical

solution of the problem as ever. If any of you can discover a process by which you can rapidly and cheaply remove from ordinary sewage all organic matter, whether soluble or insoluble, and all phosphatic salts, you will have almost solved the problem, and may expect to reap an abundant harvest both in honour and profit, besides securing the blessing of the "peacemaker," for surely he is entitled to that title who introduces harmonious action among natural laws where there was previous strife and consequent evil.

I will not detain you by describing any of the processes that have been tried to effect this end, for their name is legion, and I am not aware of one now in operation on a large scale which at the same time effects the end I have named and can be proved to be a financial success. Excepting the old plan of irrigation, all plans yet tried have been, practically, failures. The atmospheric conditions most conducive to a healthy life are pretty well known, and hitherto the evils arising from sewer emanations have been combated (when combated at all) by disinfectants, some based on their oxidising, and others on their deoxidising powers. The best disinfectant, other things being equal, is of course that which shall come into most intimate contact with the thing to be disinfected; therefore, for air pollution gaseous disinfectants must be absolutely essential, while for the disinfection of solids or liquids, disinfectants in a liquid form may be most effective. I mention this subject here to draw your attention to the mischief that may be done by the use of materials which are often puffed as being applicable to air disinfection, when their only possible use can be for the disinfection of solids or liquids, and even for this the use of some of them is questionable. In connection with this question it may fairly be asked why sewers should be allowed to poison the air with their gaseous emanations; surely the engineering skill of this country is equal to the task of so ventilating sewers that every opening shall draw air in, instead of allowing poisonous gases to issue from them. Newcastle, from the inclination of its sewers and cheapness of fuel, seems to be peculiarly well adapted for such ventilation.

As part of this question, the manufacture and purification of coal gas have a peculiar claim on our consideration from circumstances to

which I need not further allude; whether the balance is in favour of iron or lime purification is a subject we might profitably investigate, and also the relative merits of each in connection with the somewhat peculiar situation of our own gas works. How far the existence of gas works in large towns is compatible with health and comfort is a question which will require a combined medical, chemical, and mechanical knowledge; some other towns appear to have settled the question in the negative, as a few days ago I saw most extensive and expensive preparations making to remove a gas work from a part of London where the population is very dense, to a distance of 7 or 8 miles down the Thames.

An examination of the water supplied to our town would furnish a very excellent paper and one that would probably give rise to some valuable discussion.

If I mistake not, such chemical examinations would place our water supply in a more favourable position than many would suppose; you may gather my opinion from the fact, that after a chemical examination of many springs of water near my own residence, I have given the preference to the Newcastle Company's water as a beverage. Beyond an occasional turbidity, which is easily removed, for drinking purposes, by a small filter, or even by subsidence, there is little to be complained of as a rule. This opinion I ask some of you to bring to the test of experiment, and as the experiments are not very difficult, I trust some of our members will make it the subject of a paper during the session.

I shall not risk wearying you by further dwelling on these subjects, as you may consider them too general in their nature, but I do ask you most earnestly to prepare papers on these, or similar subjects, as well as papers describing processes in which you find your daily work, *especially* describing any alteration or improvement in any known process, and, more important still, stating defects and needs in existing processes, so that discussion might be provoked, and thought be directed to useful ends. For instance, our medical members (of whom I am sorry we have so few) might give us some most interesting and useful papers on the germ theory of disease, and follow up the researches so ably carried on by Dr. Angus Smith and

others. To some of you may, perhaps, fall the honour of proving whether or not, zymotic diseases are propagated by living germs, and, it may be, proving that each disease has its specific germ, requiring a specific soil or condition for its germination or evolution.

The place of dialysis in the phenomena of life would also be a most interesting subject for investigation, or even the gathering up of all the information known on the subject would furnish a most interesting paper. The physics and chemistry of coal-mining would also be an interesting and useful channel for our thoughts and action, and one peculiarly appropriate to a Newcastle Society. The cause of the high temperature of coal-mines, whence is it? The opinion that it is due to mere depth is, I think, disproved by the fact, that salt-mines, nearly as deep as the Monkwearmouth coal-mine, are at a much lower, in fact, a comfortable working temperature; perhaps, investigations on the part of some of our members connected with the working of coal-mines would show that the high temperature is caused mainly by a chemical action still going on in the coal itself. The important problem of producing a light which shall be safe under the varying conditions of coal-mining is yet unsolved, and any of you who can discover a practicable mode of generating light without heat, or with such a moderate degree of heat as would fail to ignite carburetted hydrogen would solve the problem, and be hailed as one of the world's greatest benefactors. A safer mode of getting coals than by the use of gunpowder is much needed, and any substance you could discover for this purpose that should have a great and rapid expansive force, combined with a moderate elevation of temperature, say below 1000° to 1200° of Fahrenheit, would bring both honour and profit to its discoverer, and confer an almost inestimable boon on the coal miner. The manufacture of pig iron has been so very ably treated here and elsewhere by your ex-President (whose eminent fitness for the work has been unanimously conceded), that it would be presumptuous in me to do more than allude to it, but he would be the first to tell you that there is room enough for all the ability and time you can devote to the consideration of that process. Especially in the matter of heating the blast is this true, while in the process of manufacturing pig into malleable iron and steel the Bessemer process has

not exhausted improvement. The limited class of irons to which that process is applicable still leaves the field comparatively unoccupied. The enormous amount of fuel used in this process above that indicated by theory should engage the attention of such of you as have the combined chemical, mechanical, and physical knowledge, and the field is ample notwithstanding the many experimenters now occupying it. What is the essential difference between iron and steel, is it chemical or physical, or both? would furnish an interesting and useful paper from some of our members engaged in metallurgical chemistry, and would probably also give rise to considerable discussion.

The alkali manufacture may be said to still await the coming man. I will not say to introduce a shorter or more economical process than the present round-about process of Leblanc, as that would lead me into an examination and description of the almost numberless processes which have been proposed to effect that end, but rather to perfect that process. How far we are from having developed all the apparent possibilities of Leblanc's process may be judged by the results obtained in practice when brought to the test of its theoretic possibilities. Taking the best commercial sulphate of soda to contain 97 per cent. of pure sulphate, all alkali manufacturers will agree with me that the amount of available soda obtained from such sulphate falls far short of that which theory indicates ought to be obtained. To make this clearer to you I have put in a tabular form what is actually got, and what we should have by theory.

100 of Commercial Sulphate at 97 per cent. NaO,SO^3 contains 42·3 Soda ($NaO = 31$).

Soda Ash at 52 per cent. Soda.			Ash at 54 per cent.	
To obtain of Soda Ash from 100 of Sulphate.	Shows loss of Soda (NaO).	Equivalent in Soda Ash.	Loss of Soda.	Equivalent in Soda Ash.
70	5·9	11·3	4·5	8·3
71	5·4	10·4	4·0	7·4
72	4·9	9·4	3·5	6·5

From this table you will observe that, taking the best commercial sulphate to contain 97 per cent. of sulphate of soda, we should obtain from it 42·3 of soda (NaO = 31). Now, in practice, different manufacturers obtain varying results, but they will be about covered by the figures in the table. Thus, some manufacturers obtain a soda ash containing 52 per cent. of soda, and a yield of 70 per cent. from the sulphate used; in this case they are short of the theoretic amount they should obtain from 100 of sulphate, 5 9 of available soda or 11·3 of such ash. If 71 per cent. of a 52 per cent. ash is obtained from the sulphate used, then are they minus 5·4 of available soda or 10·4 of such ash, and at 72 per cent. from sulphate (which is a quantity scarcely if ever exceeded) they are minus 4·9 of available soda or 9·4 of ash. To pass to the next division of the table—taking the work of a manufacturer who is making a soda ash containing 54 per cent. of soda, we still find there is ample room for improvement; thus, taking his yield of ash from sulphate at 70 per cent. (which is, I believe, common at this strength) he is (for each 100 of sulphate used) minus 4·5 of available soda or 8·3 of such ash; at 71 per cent. yield of ash he is minus 4·0 of available soda or 7·4 of such ash; and at 72 per cent. of ash from sulphate he is still minus 3·5 of available soda, or 6·5 of ash, containing 54 per cent. of soda. Now, we know that a considerable portion of this loss of soda occurs in the furnaces and lixiviating tanks, but how it occurs there, or in what form some portion of it exists in the tank waste, has not been satisfactorily shown. To prevent misunderstanding here, allow me to observe, what many of you are, I have no doubt, aware of, that the largest portion of this loss of available soda is due to the imperfect decomposition of sulphate of soda in the ball furnace, and also (but I believe in a much less degree) to imperfect lixiviation of the balls. Probably as much as 4 of soda in the 52 per cent. ash, and 2 of soda in the 54 per cent. ash, is due to these causes. And here I would urge upon such of our members as have time and opportunity to institute some experiments of research in this direction, and let us have the scientific result of their labours.

To those of you who are more directly engaged in analytical chemistry I would suggest the institution of researches, having for

their object, to explain and so go far to prevent, the varying results we get in the examination and valuation of the material used and made in our various manufactories; all of you who are actively engaged in manufactures will know the great annoyance we have from this source.

As my warrant for the observations I have just made, I beg to draw your attention to this tabulated result of the examination of seven different samples of sulphur ore (pyrites) by three different chemists, each having the same sample to operate on.

No.	Sulphur per Cent.			Difference between A and B	Difference between A and C
	A	B	C		
1	38·8	38·0	40·2	0·8	1·4
2	40·9	40 9	42·7	0·0	1·8
3	39·6	39·1	41·1	0·5	1·5
4	39·3	39·5	40·7	0·2	1·4
5	41·6	41·4	43·5	0·2	1·9
6	38·2	38·0	40·0	0·2	1·8
7	39·7	39·2	41·1	0·5	1·4
Average	.....	.....	.....	0·34	1·6

You will observe that between A and B, there is comparatively little difference, the maximum amount being 0·8, while between A and C, the maximum difference is 1·9; the average difference between A and B is only ·34, but the average difference between A and C is 1·6.

I ought to mention that A and B are local chemists, but residing and working in different parts of the district, and acting quite independently of each other, and that C is a metropolitan chemist; also that A and C are professional analytical chemists; B is chemist in the laboratory of the purchasers of the ores. I need not pursue this branch of the subject further, although many of you are aware that my observations would apply with even greater force to manganese

and other materials used in the manufactures of the district. If I had time, and the requisite knowledge, I might go through all the manufacturing operations of the district, and point out in each the need and opportunity for useful work. I trust these observations will induce you to give what aid you can in the development of our infant society, so that it may take a place among the scientific societies of our land worthy of the manufacturing district in which we are placed. In justice to myself, a few words more, and I have done. Some of you may have imagined that most of my recommendations have a mere money value for their end, and have not been made in the interests of pure science. If this is so, it is not, I assure you, because I undervalue the pursuit of science for its own sake; for I believe that such of you as devote yourselves to purely scientific investigations, from the promptings of a pure and devoted love, may not, as your reward, gain material wealth, but you will have formed, what is of infinitely greater value, a noble character, and a *capacity* for enjoyment which mere wealth cannot give.

Mr. I. L. Bell — Mr. President and gentlemen, I am sure you will at once divine the object which I have in rising to address a few words to this meeting; it is to tender our very hearty thanks to the President for the very admirable and painstaking address which he has just delivered. I trust the advice which he has tendered to the members of this society, and I will say more particularly to the younger members of the society, will be taken note of, and that we shall at no distant period be able to say that we have reaped some advantage from the address which he has placed before us in so pointed, and indeed I may say in such eloquent, terms. No doubt the address itself has partaken, as our President has justly stated, in a great degree of the utilitarian phase; but I ventured, in an address which I had the honour of delivering before this body, to state that it could scarcely be expected of gentlemen engaged, not in the pursuit of abstract science, but rather in the application of science to the general purposes of life, that any great or important contribution could be afforded by us to abstract science. But notwithstanding, it cannot be

denied that of all classes of the community there is none so capable of affording knowledge of a particular kind as we ourselves, engaged as we are in the application of those great scientific truths which have been taught to us by the chemists of the day—engaged as we are, as I have stated, in the application of those truths to practical purposes. Possibly, indeed certainly, there can be no application of science of a more interesting character than that where it seeks to elevate the standard of public health; and I am glad indeed that our President has drawn your attention to this subject. I will only state in reference to this, and in justice to a body with which I have the honour of acting, namely the Town Council of Newcastle, that to a certain extent the recommendations of the President have been forestalled by that body, because we have, I am glad to say, an accomplished chemist (Mr. Pattinson), a member of the Newcastle Chemical Society, from whom we receive periodically reports on the quality of the water supply to the town as well as on the quality of the gas; and I take this to be a very important function, indeed, of every municipal corporation, that in a matter affecting, so directly as it does, the public health, every pains should be taken to assure to those who consume the water an article of the highest possible purity. In conclusion, I would ask the meeting to do that which I have no doubt they will be too happy to acquiesce in, namely, to record a vote of very hearty thanks to the President for the very able address with which he has just favoured us.

The proposition was carried by acclamation.

The PRESIDENT—I have to thank you, gentlemen, for the very hearty manner in which you have responded to Mr. Bell's proposition. It will always give me the greatest pleasure to contribute what I can to the prosperity of this society; and, with Mr. Bell, I do hope that the younger members of this society will not be diffident in preparing papers to submit to the society. We want some young and fresh thought; and although the ideas may sometimes be crude, yet they may give rise to very useful discussions, discussions which may in the end prove of value where the papers themselves might perhaps not be of value. I thank you heartily, gentlemen.

Mr. R. Calvert Clapham then read the following paper:—

AN ACCOUNT OF THE MANUFACTURE OF COPPER ON THE TYNE.

The manufacture of copper, as now extensively carried on, has introduced a new industry into this district. Up to the year 1850, no copper was produced on the Tyne; in 1851, J. and W. Allen commenced soda works at Wallsend, to work out Mr. Longmaid's patent, and they extracted as much copper as produced 4 tons sulphate of copper weekly; a few years later, Mr. Gossage erected copper works at Willington, Mr. Russell at Walker, and Mr. Mease at Jarrow, to work up the poor copper ores then chiefly obtained from Cornish and Irish pyrites. The regulus, or precipitate, was sent to Swansea, to be smelted for copper. H. L. Pattinson and Co. began to smelt copper ores, obtained from burnt pyrites, in 1858; the regulus also being sent to Wales. Up to this date the total copper regulus, etc., made on the Tyne, would not represent more than 400 tons of copper annually.

In 1865, copper works were erected at Hebburn, to carry out the so-called Henderson process, for extracting copper by the wet method, from ores obtained from Spanish pyrites. Since that date there has been a rapid increase of production in the older works; and, recently, others have been erected for a similar purpose by the Bede Metal Co. at Jarrow.

The total quantity of copper produced on the Tyne in 1869 was 4,100 tons, and of this quantity about 280 tons consisted of sulphate of copper, and the value of the products was about £340,000. This production has been greatly extended in the present year. From the facilities now at hand for the disposal of the burnt copper pyrites produced in the district, instead of sending them all the way to Swansea, and the cheap rate at which copper pyrites can be supplied, it appears pretty certain that this ore will soon take the place of non-cupriferous ores. The total consumption of pyrites, of all kinds, in the United Kingdom, for 1869, was nearly 400,000 tons, namely:—

Imported from Norway	63,091	tons.
„ Holland	13,983	„
„ Portugal	140,805	„
„ Spain	99,648	„
„ Other parts	2,420	„
	319,947	„
Produced in Ireland	56,291	„
„ Cornwall and other parts	19,658	„
	395,896	„

and of this quantity 265,453 tons consisted of copper pyrites, namely:—

Imported from Norway	25,000	tons.
„ Portugal	140,805	„
„ Spain	99,648	„
	265,453	„

Large as this importation is, it may be interesting to know that the importation of pyrites for this year will not be far short of 400,000 tons. The importance of this fact on the future development of the soda trade cannot be exaggerated, and indeed these manufactures—soda and copper—are every year becoming more dependent on each other. The erection of copper works in this district has already had a marvellous effect in increasing local importations, and at the same time reducing the cost of the sulphur to the soda maker, the price having fallen 40 per cent. since 1865.

As the plan of manufacture generally conducted in the district is well known to most of the members, I need only briefly refer to it. The pyrites most suited for the purpose is obtained from Spain, and contains less silica and other impurities than most other varieties.

In the first place the sulphur is nearly all extracted by the soda maker in producing his sulphuric acid; the ores are then sent to the copper-works: at this stage the better kinds contain about 3 to 5 per cent. copper, 4 per cent. sulphur, 4 per cent. silica, small per centages of silver and lead, and the balance is peroxide of iron. It is mixed with a certain weight of common salt, the quantity depending on the per centage of sulphur left after burning. The whole is ground to a fine

powder, and is then placed in long reverberatory furnaces, which are kept at a moderate heat. During this operation the sulphur is converted into sulphuric acid, and the salt is at the same time decomposed; the hydrochloric acid acts upon the copper, and, provided the heat in the furnace is not kept too high, the whole of the copper is rendered soluble, and can be washed out of the residue. A part of the chloride of copper is, however, driven off by the heat, and passes off into the condensers along with the free hydrochloric acid, and is there condensed. The acid solutions from the towers is of a distinct greenish blue colour, showing the presence of copper.

When the ground material in the furnace is complete, it is thrown into wooden tanks, and digested in water and the acid solutions from the condensers, the acid assisting most materially in dissolving out the metals. The liquors run off from the tank are necessarily of a mixed nature, and consist of chlorides of copper, silver, and lead, and undecomposed common salt, also sulphates of soda and lead, &c.

To obtain the copper, the liquors are treated with old scrap iron or spongy metallic iron, which is made from the residue ores, by heating with coal in a muffle furnace. In both cases the copper is precipitated, but when spongy iron is used the action is much more rapid. The precipitate thus obtained is repeatedly washed in water to free it as much as possible from the salts of iron, soda, &c., after which it is allowed to drain and partially dry. In this state, containing about 70 per cent. of copper, it is smelted in an ordinary reverberatory furnace, the first smelting yielding what is termed pimple copper. This is again slowly melted in a similar furnace, with a free current of air passing over it for the purpose of oxidizing the impurities; it is run out and forms what is termed "blister copper," which is refined into "cake or ingot copper" for the market.

The residue left in the tanks consists of nearly pure peroxide of iron, and is extensively employed for fettling in puddling furnaces and in blast furnaces in the place of hematite ores. Its composition may be roughly stated as follows:—60 to 65 per cent. metallic iron; 2 to 5 per cent. silica; 5 to 15 per cent. water, the latter depending on the quality of the ore and the time it remains in stock.

Most of the Spanish pyrites contain also notable quantities of both silver and lead, and in some cases gold; but until very recently no attempt has been made to separate these metals. Mr. F. Claudet has, however, patented a process to effect this object, which may be briefly described as under. The ore is treated as above described: the first two or three washings, Mr. Claudet states, contain the silver, the chloride of silver formed during the calcination being rendered soluble in the large excess of common salt which is used. The solutions to be treated for silver are run into large vats, where a soluble iodide is added to precipitate the silver. The precipitate thus collected is a mixture of iodide of silver and sulphate of lead, with salts of copper. The latter are dissolved out by weak acid, and the remaining precipitate is decomposed by metallic zinc, which reduces the iodide of silver. The zinc iodide, which is formed, is used to precipitate chloride of silver from other solutions. The precipitate also contains gold.

It will be seen from the above short statement, that all the metals in the ores can be extracted and made commercially useful; and that the only article produced in any quantity, and not hitherto utilized by the process, is sulphate of soda. Experiments upon a large scale have already been carried out by Dr. Merz, at the Tharsis Sulphur and Copper Company's Works, to recover it from the waste liquors; and it may reasonably be expected that in time it also will be obtained.

But if we leave our own locality, and glance for a moment at the production of copper in the United Kingdom generally, we find that the native mines, which have been worked for centuries past, reached their maximum production in 1860, viz., 15,968 tons. As the importations from abroad increased, and the production of copper from burnt pyrites gradually assumed greater importance, the mining of native ores has been proportionately abandoned, and the weight raised in 1869 amounted to only 8,291 tons, equal to a reduction of nearly 50 per cent.

The following figures represent the production, importation, exportation, and consumption of copper in 1869 :—

Copper made from native ores			8,291 tons.
Viz., from Cornwall and Devon ...	6,794 tons.		
„ Wales	346 „		
„ Isle of Man	30 „		
„ Ireland	1,022 „		
„ sundry precipitates ...	99 „		
	8,291		
Copper made from burnt pyrites		7,600	„
Viz., from the Tyne	4,100 tons.		
„ Lancashire and other places	3,500 „		
	7,600		
Imports (consisting of ores, regulus, bars, rods, &c., calculated as pure copper)		63,097	„
		78,988	„
Viz., from Chili	42,000 tons.		
„ Australia	7,500 „		
„ Cape of Good Hope, Norway, North America, and other places	13,597 „		
	63,097 „		
The exports of copper manufactured in England are ...		42,569 tons.	
Do. foreign copper		12,309 „	
		54,878 tons.	

The consumption of copper in England was as under:—

1864	20,120 tons.
1865	26,514 „
1866	28,326 „
1867	20,305 „
1868	17,400 „
1869	21,665 „

Or an average of 22,388 tons annually.

In conclusion, I have to thank the copper manufacturers for the prompt manner in which they have given any information required, and Mr. R. Hunt, F.R.S., for valuable information.

The PRESIDENT—I think you will all agree with me that this useful statistical and practical paper contains an amount of information which is not only valuable to us as a society, but which will be found to be very valuable to both manufacturers and merchants having dealings with copper. If there is any gentleman present who is conversant with the trade, especially with the extraction in the wet way, we will be very happy to hear any observations which he may have to make on this paper.

Mr. BELL—I should like to ask Mr. Clapham if he has any information as to the causes which led to the quantity of copper consumed in this country falling off from 28,326 in 1866 to 17,400 in 1868.

Dr. LUNGE—Is it possible to calculate the consumption in this country with anything like the accuracy that the exportation can be calculated? It is quite possible that some of the returns have not been received.

Mr. BELL—Yes; I take it that the consumption is a mere question of subtraction; that is, there was so much copper imported, so much made, and so much exported; and the two first being added, and the second subtracted, gives a sum assumed to be consumed; of course, that might be fallacious, as Dr. Lunge says, only it appears extraordinary that this should amount to nearly 50 per cent.

Mr. PATTINSON—But I think there is really a diminution of the amount of copper used in this country from the fact that the use of copper in vessels is going out, owing to the large extension of the application of iron to the building of ships. I know it has affected the ship-building trade, that is, as to the consumption of zinc for Muntz's metal, and, of course, it will affect copper as well, in fact to a much greater extent.

Mr. NAPIER—I might say that so far as silica is concerned Mr. Clapham has taken the average of the ores used on the Tyne. Mason's ore, for instance, will contain perhaps not 2 per cent. of silica; other ores may run to 6 or 7 per cent.; so that if you take the quantity burnt at what Mr. Clapham has put down at 4 per cent. as the average of purple ore produced on the Tyne, it will not be very far wrong.

Mr. PATTINSON—How much water did you find?

Mr. NAPIER—That depends entirely on circumstances. If it goes direct from the tanks in which the operation is conducted it will contain a considerable quantity.

Mr. PATTINSON—How much?

Mr. NAPIER—That depends upon the ore itself. It depends upon the kind of ore that has been worked If you take Norwegian ore you will find it to contain 20 or even 30 per cent. The one contains so much more water than the other.

The PRESIDENT—The proportion of water must necessarily be very variable

Mr. BERKLEY—I have had a good deal through my hands first and last, and we generally allow from 16 to 20 per cent. of water, and it comes straight from the tanks. The metallic iron is generally about 64 or 65. I think its principal use will be for fettling puddling furnaces. I do not know whether Mr. Bell has used it for his blast furnaces or not [Mr. BELL—No], but I have had a great deal of difficulty in using it in large quantities. I have made various experiments on burning it to run it together, and have succeeded partially but not to any practical extent. If we could get to any system of running it together in lumps and getting rid of the moisture, then it might be used for blast furnaces; but at present it can only be used in small quantities. But for fettling furnaces I think it is coming into very extensive use, and will lessen the price of fettling very much. Mr. Clapham said that it can be purchased at something like 14s. when hematites are something like 24s. There is one subject which Mr. Bell alluded to, the cheapening of copper. I believe it is down to 60s. or 65s.

Mr. BELL—No; it is rather up as a fact. Ores are very much dearer than what they have previously been.

The PRESIDENT—Copper ores?

Mr. BELL—Yes.

Mr. BERKLEY—I think sulphur ores, containing copper, are rather cheaper. I may say they are a little dearer now than they were last year: a shade up, I believe.

Mr BELL—With regard to what fell from the last speaker, about

the use of this material for making iron, treating it in the way he mentioned, there is one great inconvenience: the more completely you frit an ore of iron before putting it into a blast furnace the more difficult it is of reduction. I do not need to say a moderate quantity can be so treated, but the more porous and open you have the material you are using in the blast furnace I believe the better it is.

Dr. LUNGE—In reference to this, will you allow me to make a remark? I have been myself interested in a patent which had precisely this object in view. We ground our purple ore together with coal dust of such a nature that it would give a good coke; then we got really a very good nice hard coke which contained iron in a metallic form. This we intended to mix with the ordinary ores in the blast furnace and in that way to utilize it. We did not go very far in it, because I found that a fortnight before another patent embodying this entirely had been applied for; so we gave up every thought of going further into the matter; and I do not know whether there is really anything in it: it never went beyond the laboratory stage.

Mr. BERKLEY—I have seen coke tried with ore made into coke, but I always found it would not carry any burden. There was no solidity in the coke, it was so very easily broken; that was the great objection we found against it. In reference to getting the ore into a solid form, I did not allude to that, but just simply to a sort of spongy state.

Mr. BELL—I believe that all those patents which have for their object the saving of the work of blast furnaces, by introducing iron already in a metallic form are founded on a fallacy. I believe there will be no kind of advantage accruing from this mode of treatment.

Dr. LUNGE—But that was not our object or our intention at all. It was a thing that we did not seek for. We found it as a matter of fact apart. We never found that the whole of the ore was reduced. We could extract it with a magnet very easily—a great part; but that was not our object. Our object was this, to prevent the fine purple ore from stopping the insterstices or channels in the blast furnace; and, of course, it would have required a very extensive practice of trials to find out a proper mixture which could have gone

into the blast furnace, as Mr. Berkley says. We, of course, had not that in view, finding that the whole of our labours would be in vain, because this had been patented before we gave up.

Mr. BELL—I believe sincerely that you owe a debt of gratitude to the gentleman who prevented you going any further. Mr. Berkley is quite right: we all know that unless you have coke of a certain character as to hardness, it is not of much use in a blast furnace. Now, we know perfectly well that the moment you pass a certain amount of ash in coke, you cease having it of the requisite consistency. Whether you put in that ash in the form of an oxide of iron, and reduce it to the metallic state, or magnetic oxide, or whether that ash exists naturally in the coal, I believe it will be found to be attended with the same result, namely, the production of a coke, which, as Mr. Berkley styles it, has no body in it.

Dr. LUNGE—Yes, but it is a question of quantity.

Mr. BELL—Of what quantity?

Dr. LUNGE—Of the quantity which had been introduced into the blast furnace. It does not follow that you should work your blast furnace entirely with such coke as that.

Mr. BELL—But you are merely concealing the evil by mixing it. It is there.

The PRESIDENT—Yes.

Dr. LUNGE—It may not have such an evil effect on the blast furnace.

Mr. BELL—But it will. You will have this difficulty: every piece of soft coke which you introduce into the furnace produces a result, even if it is a fractional part of what it would be if you introduced it entirely. It might be so small that you could not estimate it, but it is there for all that.

Dr. LUNGE—Then it is a question which point is of more value. At that time the purple ore had no value whatever. It was lying in immense heaps everywhere on the banks of the Tyne, and it was exceedingly desirable to find some use for it. We were well aware that, so far as it went, the crumbling property would be a disadvantage; but the advantages to be reaped from it might have been

greater. I do not put it forward as having any merit of its own, but merely to reply to the question of Mr. Berkley.

Mr. BELL—You have an awkward competitor in the Cleveland Hills. There is very little margin.

Mr. BERKLEY—The cost would preclude any general use.

Mr. BELL—A ton of iron made on the Tyne from Cleveland stone does not cost above 15s. or 16s. for the ore. If you take your purple ore as containing 60 per cent., that is a ton-and-a-half would go to make a ton of coke, this at 16s., will give 24s. per ton of iron for ore; of course this might be met if you produced a quality of metal for which you could get very much higher prices. If you use a small quantity along with the Cleveland ore, you then must be content to take the Cleveland price for it, and substitute the Cleveland ore by your purple ore.

The PRESIDENT—The discussion on this paper has been very interesting, and I hope useful, to our metallurgical members; and I am glad Mr. Bell is present. He has had very large experience in iron manufacture, and his remarks are peculiarly valuable on a question of this kind. The metallurgical part of Mr. Clapham's paper is peculiarly valuable, and I have no doubt will be found useful.

Mr. BERKLEY—I think we should move a vote of thanks to Mr. Clapham for his paper.

The motion was carried by acclamation.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, JANUARY 26TH, 1871.

JOHN GLOVER, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Messrs. William Crowder and Henry Langdale were unanimously elected members.

The name of Dr. Adolph Hess, Mushroom Chemical Works, was read for the first time.

Mr. J. W. SWAN read :—

ON A NEW FORM OF ANEMOMETER.

THE instrument which I wish to bring under the notice of this meeting, is a modification of Fletcher's Anemometer, and, like it, is designed more particularly, for the measurement of the speed of currents of air, in the flues and chimneys of chemical and other manufactories. With Mr. Fletcher's description of his Anemometer, and his interesting researches on the speed of air in flues, published in the report of the inspector under the Alkali Act, you are all doubtless familiar. On the table are both Mr. Fletcher's Anemometer and my own, and with your permission, I will now proceed to point out, the characteristics of each, and the manner in which they differ.

Mr. Fletcher's Anemometer consists, as you will perceive, of two glass cylinders, three inches high and three inches in diameter, placed side by side, and connected by a tube about one-tenth of an inch in diameter, opening into the centre of the base of each; the whole being equivalent to a U-shaped tube of large dimensions, considerably contracted at the bend. These cylinders are partly filled with ether, and in each is a metallic float. Between the two cylinders is fixed a

scale and vernier, by means of which, and the floats, any variation in the levels of the liquid contained in them, can be accurately measured. When in use, each of these cylinders is connected, by elastic tubing of any convenient length, with a tube, placed in the current of air, the velocity of which it is desired to measure. One of these tubes is straight; the other has its end bent round at a right angle.

My own Anemometer, also consists of a U-shaped tube, but of very small diameter, it being only about one-eighth of an inch in the bore. This is very much lessened at the bend, in order to diminish the tendency to oscillation of the liquid it contains, which is ether. The tube is ten inches in length, and, as you will observe, is not placed vertically, as in Mr. Fletcher's instrument, but is laid at an inclination of 1 in 10. Each limb is furnished with a scale of inches and a vernier, by means of which, the difference of the levels of the liquid in them, can be read off easily and accurately, to one-hundredth of an inch. The verniers are partly of glass, and overlie at once the scale and tube, enabling the reading to be taken with great precision. The ends of the tube, are terminated by a two-way stopcock*, by which their communication with the tubes placed in the current, can be reversed without disconnecting any portion of the apparatus. The whole is mounted on a suitable stand, furnished with a level and levelling screws. The tubes for placing in the current are the same as Mr. Fletcher's, except that they are made of glass, instead of metal.

Now, in the principle of their action, both of these instruments are identical. In each, the moving force of the current of air, is used as the measure of its velocity; and in each, the method of estimating this force is the same; but in the plainness of their indications, they widely differ, and it is in this respect that mine has, I think, an important advantage. The differences in vertical height, of the columns of ether, corresponding to velocities of air, equal to 4 or 5 feet per second, are ·02 and ·03 of an inch respectively; these are very small quantities, so small indeed, that even with the facilities, afforded by Mr. Fletcher's scale and floats, they require more care and patience

* I am indebted to Dr. Wright for the suggestion of the two-way stopcock.

to ascertain them, than most persons would care to bestow. For this reason, for velocities below 4 or 5 feet per second, Mr. Fletcher's instrument becomes practically useless. By placing the tube at an inclination of 1 in 10, as in my Anemometer, instead of vertically, its indications are increased tenfold; the column of ether, having to occupy ten times as much space obliquely, as is equal to the vertical height it represents. This is at once a manifest advantage, not only in measuring currents of low velocity, but also for all speeds, and will be duly appreciated by any one having occasion to make use of an instrument of the kind. Inclining the tube, in order to obtain an increased indication, is by no means a new idea, it having long since been applied to the Barometer; but the adaptation of the principle to the Anemometer, was, as far as I am aware, first made by myself, it being now nearly two years, since my experimental apparatus, embodying this improvement, was constructed. The use of an inclined tube, necessitates its being of small diameter. The bore which I have found most suitable, being about one-eighth of an inch. Objection has been raised, to the use of small tubes, for making these measurements; on the ground, that the resistance they offer, to the motion of the liquid within them, is sufficient to impair, the accuracy of their indications, and Mr. Fletcher, accepting this view as correct, expands the limbs of his U tube, into wide cylinders, in order to decrease friction. I have, however, proved by experiment that when the fluid they contain is ether, such an objection is groundless. The adhesion of ether to glass, is so exceedingly small, that a bead of it weighing only ·003 of a grain, contained in a capillary tube, overcomes it, and descends in the tube by its own weight. Further; on comparing Mr. Fletcher's Anemometer, side by side with my own, under precisely equal conditions, their indications were found to be practically identical. The use of wide cylinders, is therefore, unnecessary; they also render the construction of the instrument costly, and its repair difficult. What is required for practical purposes is an instrument, the indications of which are reliable, and easy to be perceived; not too complicated in construction, that it may be easily repaired when injured, and not to costly. These essentials, it has been my aim to supply.

It may now, perhaps, be interesting, to make a few remarks, on the principles of the action, of these Anemometers, and the method of using them.

As was before mentioned, the indications of these instruments, represent the moving force, of the current of air, to the action of which they are subjected. This force is measured, by the difference in vertical height, of the levels of the columns of ether, in the U tube; and, from it, the velocity may be determined by observing, that according to the law of bodies in motion, their velocities vary, as the square root of the pressures; and, that air, at a temperature of 60° Fahrenheit, moving at a velocity of 28·55 feet per second, exerts a force, equal to the pressure of a column of ether, of one inch in vertical height. The fact, that air moving at a speed of 28·55 feet per second, is equal to a pressure of one inch of ether, was established by Mr. Fletcher, from a series of very careful experiments, and, as it agrees, with results, obtained from purely mathematical considerations, may be taken as correct. In measuring currents, arising either from diminution of density, as in the action of a chimney; or from increased pressure, as in a blast pipe; it is necessary, that both terminals of the U tube, should be placed in it, at right angles to its direction, and, with the opening of the bent tube, directly facing it. Were one terminal only employed, the other being left open to the atmosphere, the pressure, represented by the liquid in the U tube, would be due, not only to the moving force of the current, but also to the difference between its density, and that of the atmosphere. Again; if both terminals were alike, and placed under equal conditions in the current, the action upon one, would be neutralised by that upon the other, and there would be no motion in the liquid in the U tube; but, by bending one, and presenting its opening to face the current, and placing the other at a right angle to the current, the action upon them becomes different; the bent tube, receives the impact of the moving air; the straight one, has a partial vacuum created in it, by the air rushing across it; and both these forces, acting simultaneously, and in the same direction, the liquid in the U tube, is raised to a height, corresponding to double the moving force of the current. Currents of air, moving in confined spaces, are found, to have a greater

velocity, at the centre, than at the outside. In a circular flue, or chimney, the mean of speed, occurs, at about one-sixth of its diameter. This point, must have attention, in placing the tubes in the current. In using my Anemometer, it must be set level, before being connected with the tubes in the current; the height of the column, in each limb of the U tube, is then read off, by the scale and verniers; and the required difference found, by subtracting the lesser height from the greater. The necessity for levelling it in both directions, may be dispensed with, by taking the reading in one limb only, which is done in the following manner:—The instrument, being connected with the tubes in the current, the level of the liquid in the one limb, is noted; then, by means of the stopcock, the connection with the tubes is reversed, and the level in the same limb, again observed; the less height, subtracted from the greater, gives, as before, the required difference. In this case, however, as well as in the other, it must be levelled in the direction of its length.

Mr. Fletcher, has calculated a table of velocities, corresponding to different vertical heights, of the ether in the Anemometer, some of the principal of which, are given below. In applying this table to my Anemometer, the reading of it, must be divided by 10, in order to reduce it, to the equivalent vertical height. The table is constructed for atmospheric air, at a temperature of 60° Fahrenheit, at which, and the standard barometric pressure (29·92 in.), the measurements with the instrument, are supposed to be made. Air usually forms, the bulk of the current, in chimneys, but when it is largely mixed with such gases or vapours, as materially alter its density, or if its density, is either diminished or increased, by the temperature being above or below 60° Fahrenheit, this increase or decrease of density, must be taken into account, in estimating its velocity, from the reading of the Anemometer. A table of corrections for temperature is given below. The speed, corresponding to the reading of the Anemometer, multiplied by the factor, found in the column opposite the observed temperature of the current, gives the true speed per second in feet of air at the standard temperature. Small variations of barometric pressure, need not be taken into account in making the measurements; but when it is necessary, to make the correction, one of the

formulæ below, must be made use of. The following formulæ, are given by Mr. Fletcher, for calculating, the velocity of currents of air, corresponding to any reading of the Anemometer; and also the necessary correction for variations of temperature and barometric pressure:—

1. For ascertaining speed; $v = \sqrt{p} \times 28{\cdot}55$, in which, p, is the reading of the Anemometer, in inches in vertical height; gives speed in feet per second.

2. For corrections for temperature; $v = \sqrt{p \frac{519}{459 + t}} \times 28\,55$ in which t, is the observed temperature; gives speed per second in feet of air at 60° Fahrenheit.

3. For correction for barometric pressure; $v = \sqrt{p \frac{29{\cdot}92}{h}} \times 28{\cdot}55$, or $v = \sqrt{p \frac{h}{29{\cdot}92}} \times 28{\cdot}55$; the former, giving speed per second, in feet of air under the existing pressure; the latter, in feet of air, at the normal pressure.

TABLE I.

SHOWING THE VELOCITIES, OF CURRENTS OF AIR, AS INDICATED BY THE ANEMOMETER. (FLETCHER.)

Reading of Swan's Anemometer ÷ 10, = inches in vertical height.

Temp., 60° Fah. $v = \sqrt{p} \times 28{\cdot}55$. Barometer, 29·92 in.

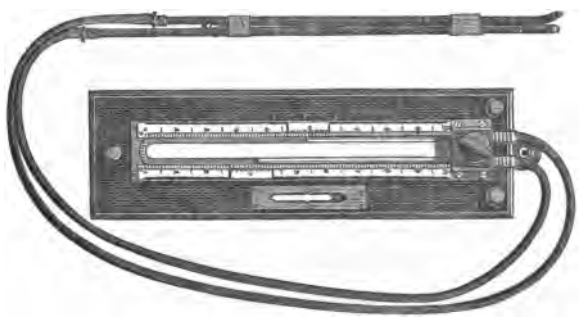
Anemometer reading. Inches in vertical height.	Speed of air in feet per second.	Anemometer reading. Inches in vertical height.	Speed of air in feet per second.	Anemometer reading. Inches in vertical height.	Speed of air in feet per second.	Anemometer reading. Inches in vertical height.	Speed of air in feet per second.
0·001	0·903	0·079	8·025	0·276	15·01	0·600	22·12
0·005	2·019	0·100	9·028	0·314	16·00	0·650	23·02
0·012	3·127	0·124	10·053	0·356	17·04	0·750	24·73
0·020	4·038	0·150	11·06	0·398	18·02	0·800	25·54
0·031	5·027	0·178	12·05	0·444	19·03	0·850	26·32
0·045	6·056	0·210	13·08	0·492	20·02	0·900	27·08
0·061	7·051	0·242	14·05	0·550	21·17	1·000	28·55

TABLE II.

CORRECTIONS FOR TEMPERATURE.

$$\text{Factor, } = \sqrt{\frac{519}{459 + \text{temperature.}}}$$

Temp. Degrees Fah.	Factor.	Temp. Degrees Fah.	Factor.	Temp. Degrees Fah.	Factor.	Temp. Degrees Fah.	Factor.
30	1·0302	110	0·9551	190	0·8943	340	0·8060
40	1·0198	120	0·9468	200	0·8875	360	0·7960
50	1·0098	130	0·9388	220	0·8743	380	0·7865
60	1·0000	140	0·9309	240	0·8618	400	0·7763
70	0·9905	150	0·9232	260	0·8497		
80	0·9812	160	0·9156	280	0·8380		
90	0·9723	170	0·9083	300	0·8269		
100	0·9636	180	0·9012	320	0·8163		



The engraving, having been taken from the instrument as it hung against a wall, does not show the inclination.

In the discussion which ensued Mr. SWAN explained, in answer to Mr. DAGLISH and other members, that the instrument was contracted in the bend for the same reason as the ordinary water-gauge, viz., to prevent oscillation; also, that, by substituting a plain tube for the double one, the instrument might be employed as a delicate pressure-gauge.

Dr. C. R. A. WRIGHT read :—

NOTE ON THE SPECIFIC GRAVITY OF AQUEOUS HYDROBROMIC ACID.

THE writer having recently had occasion to prepare some little quantity of pure aqueous hydrobromic acid, advantage was taken of the circumstance to examine the relation subsisting between the specific gravity and per centage of HBr in specimens of acid of different strengths.

The acid was obtained by the action of sulphuretted hydrogen on excess of bromine in presence of water, and subsequent repeated rectification over pulverized potassium bromide; it was free from sulphuric acid and other sulphur compounds, and contained no free bromine. The per centage of HBr was determined by a standard alkaline solution, and the specific gravity by weighing a known volume of the liquid. The results, which do not pretend to very rigorous accuracy, are as follow :—

Per centage of HBr.	Sp. gr. at 15° c.
10·4	1·080
23·5	1·190
30·0	1·248
40·8	1·385
48·5	1·475
49·8	1·515

By graphical representation, it was found that these numbers agree tolerably together, yielding a *mean curve* from which the following table is constructed.

Per centage of HBr.	Sp. gr. at 15° c.	Difference for 5 °/₀.	Difference in 1 °/₀.
Nil.	1·000		
5	1·038	0·038	0·0076
10	1·077	0·039	0·0078
15	1·117	0·040	0·0080
20	1·159	0·042	0·0084
25	1·204	0·045	0·0090
30	1·252	0·048	0·0096
35	1·305	0·053	0·0106
40	1·365	0·060	0·0120
45	1·435	0·070	0·0140
50	1·515	0·080	0·0160

From these numbers it appears that the increase in specific gravity is not proportional to the increase in per centage of HBr, showing in this respect a difference between hydrobromic and hydrochloric acid. The annexed pair of curves illustrates this difference, the hydrochloric acid curve being plotted from the table of Dr. Ure, given in "Watt's Dictionary," I, 893.

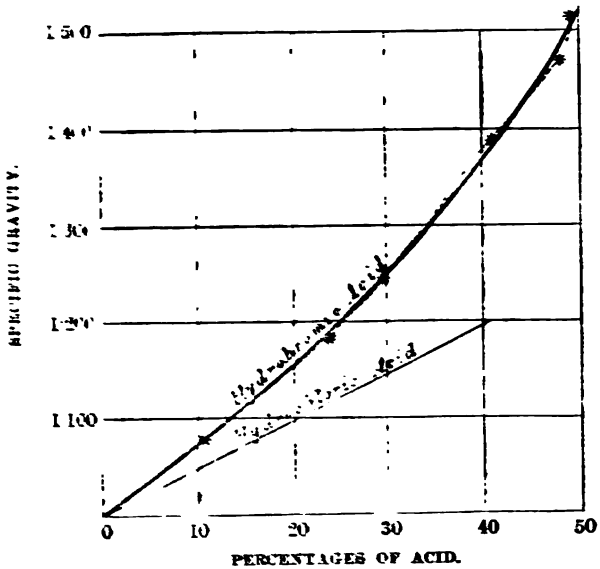
The numbers in the foregoing table agree tolerably with rough determinations given by other experimenters; thus C. G. Williams' (Jahresbericht, 1858, 438) finds acid of 37 °/₀ HBr has specific gravity 1·32: that deduced from the table for this strength is 1·305 + 2 × 0·0120 = 1·329.

In "Miller's Chemistry," 4th edition, II., 146, acid of *about* 47 °/₀ HBr is said to have specific gravity 1·486 (authority not stated); the specific gravity deduced from the table is 1·435 + 2 × 0·0160 = 1·467, the specific gravity 1·486 representing 48·2 °/₀ HBr.

In many text books, however (Watt's Dictionary, I, 673; Gmelin Handbook, II, 281; Brande's Chemistry, &c.), the specific gravity of the densest aqueous hydrobromic acid is given, on the authority of Löwig, as 1·29: if this number be correct, the solubility curve must be a very abnormal one, the curvature up to 50 °/₀ HBr being *convex* towards the horizontal base line, while at some higher per centage a point of contrary flexure will be found, after which the curve becomes *concave* towards the horizontal line, and approaches this line instead of receding from it: it would appear much more probable that the acid employed by Löwig contained a large quantity of hydrochloric acid, a saturated aqueous solution of this acid having only a specific gravity of about 1·2. As yet, stronger acid than 50 °/₀ has not been examined, but it is proposed to determine the form of the solubility curve for higher per centages of HBr.

The foregoing experiments were made in the laboratory of Messrs. Bell Brothers, Washington.

CURVES OF SOLUBILITY OF HYDROCHLORIC AND HYDROBROMIC ACIDS IN WATER.



The dotted line joining the asterisks is the line given by the experimental numbers, the other is the "mean curve" obtained by interpolation.

On the motion of the PRESIDENT a vote of thanks to the authors was passed by acclamation.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, IN THE THEATRE OF THE COLLEGE OF MEDICINE, ON THURSDAY, FEBRUARY 23RD, 1871.

JOHN GLOVER, ESQ., PRESIDENT, IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

Dr. Adolph Hess was unanimously elected a member.

Mr. A. FREIRE-MARRECO read :—

ON THE COMMENCEMENT OF A TECHNICAL MUSEUM FOR NEWCASTLE.

My object in bringing this subject before the Newcastle Chemical Society, is rather to elicit the opinion, individual and collective, of members, than to offer any special remarks of my own. That in our northern metropolis no assembled collection representative of our industries, illustrating in fact Tyne, Wear, and Tees, exists, can hardly be accepted as a satisfactory state of things. Yet there can be little doubt that such a collection might be formed, and that those among us who contributed to the International Exhibitions of 1851 and 1862, and who have enriched the Industrial Museum of Edinburgh, would not be backward in aiding an undertaking, which, if not appealing to so wide, would at least appeal to an equally appreciative public.

Entertaining these views, I should have before this have brought the subject forward in this Society's meetings, but for a difficulty which presented itself in the outset.

Given the contributions, the question arises, who will undertake their mounting and arranging, and, more important still, who will provide the means of displaying the collections where all can examine them, or in plain English, where is the house room? Twelve months ago no satisfactory answer could have been given to this, but now I believe there can. I need hardly say that the collection of minerals (for which we are mainly indebted to the munificence of Sir W. G. Armstrong) in the Newcastle Museum, is unequalled in any other provincial town, but I have long felt that its general interest would be greatly enhanced if to it were superadded a collection in which various mineral species were treated, not as such, but as "ores," and their technical applications and products illustrated.

Until now the space available for the mineralogical collection barely accommodated the existing specimens, but the alterations and re-arrangements in progress, will it is hoped leave a good deal of case space at the Curator's disposal.

I would suggest then, assuming that the members of the Newcastle Chemical Society would aid in filling these cases with technical specimens, that the officers of the Museum might perhaps make themselves responsible for their care and exhibition. As I premised, my main object at present is to elicit the views of this Society, and I now, therefore, leave the subject in the hands of the meeting for discussion.

In the discussion which followed it was suggested that the specimens might be collected by the Chemical Society and deposited in the care of the Natural History Society. It was finally agreed that Messrs. R. C. Clapham, Henry Scholefield, Dr. Wright, and the Treasurer be a deputation to ascertain the views of the Natural History Society on the subject, and report to the next general meeting.

Newcastle-upon-Tyne Chemical Society.

ORDINARY MEETING, MARCH 23RD, 1871, IN THE THEATRE OF THE COLLEGE OF MEDICINE.

THE PRESIDENT, JOHN GLOVER, ESQ., IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

The PRESIDENT read:—

ON THE ADULTERATION OF PORTLAND CEMENT.

DURING the last few days I have learned that large quantities of blast furnace slag are being ground with Portland cement in this district; and as, in the analysis of such cement, great stress is very properly laid on its percentage of hydrated silicic acid, I have thought it well to draw your attention to the fact, as blast furnace slag, on being treated with hydrochloric acid, yields a large percentage of hydrated or gelatinous silica, although it has not the slightest value as cement. You will see, from what I have said, that a chemist, proceeding to the analysis of a cement so adulterated, might be misled if he were not aware of such adulteration. As a proof that such adulteration is a fraud on the purchaser, I have caused the following experiments on the cohesive power of a pure cement, and of the same cement mixed with slag, to be tried. The average of a number of bricks of pure cement carried a weight of 485 lbs. and broke with 492 lbs., while the same cement, mixed with its own weight of slag, bore a

weight of only 301 lbs. and broke with 308 lbs. A mixture of the same, in the proportion of three cement to one slag, bore 375 lbs. and broke with 382 lbs. Each brick was made in the same mould, and was allowed seven days to harden. You will see, from these tests, that the purchaser of such cement will not be able to mix with it the same amount of sand as when he buys the pure article, as the slag already mixed with it has taken the place of so much sand, and is really of no greater value than sand, as bricks mixed with sand gave nearly the same results as those mixed with slag. If slag gives a better appearance or colour to Portland cement, a fair trader should state the fact of its presence, so that his customer might know the price he is paying for such qualities. If our analytical chemists would undertake to test the physical properties of cement, such as cohesion, hardness, &c., as well as its chemical composition, they would supply a want, as there is no public tester, at least in this neighbourhood.

This being the last meeting of the session, I have ventured to lay this somewhat incomplete paper before you, but I may possibly resume the subject next session.

Mr. Crowder—It has been asserted by a French authority that slag may be used as one of the ingredients in the manufacture of cement.

Mr. Crowder read:—

ON THE DETERMINATION OF NITROUS ACID WITH SPECIAL REFERENCE TO THE MANUFACTURE OF OIL OF VITRIOL.

In the course of my daily occupation I have had occasion to make some observations at the chambers on the absorption of nitrous acid, and being in want of some rapid but accurate method for determining the quantity in the vitriol from the columns and denitrators, I have endeavoured to devise a good process for accomplishing this object.

I cannot find that there are more than one or two persons on

the Tyne who use any quantitative process at all, most managers depending upon the rule of thumb method of adding water and judging the amount of absorption by the quantity of gas evolved on admixture.

The only methods with which I am acquainted are the "chloride of lime" and the "urea" processes. The chloride of lime process is in use occasionally, but I cannot find that any attempt has been made to express in terms of nitrous acid or nitrate of soda the percentage of nitrous acid present.

The directions for performing this analysis in Richardson and Ronalds are as follows:—

"A small tube, measuring 20 cubic centimetres, is filled with chloride of soda of 100 Gay Lussac, its contents are poured into a phial which contains about two-thirds of a pint of distilled water.

"Another glass instrument, measuring 20 cubic centimetres, divided into 200 parts, is filled with nitrous sulphuric acid to be tested, and the acid is gradually poured from it into the phial which contains the solution of chloride of soda until, on adding a few drops of a solution of indigo in sulphuric acid, the blue colour is no longer destroyed. If 17 or 20 parts only have been used 10 pints of the acid tested will cause a saving of 1 lb. of nitre. The more acid required to destroy the colour of the indigo the less nitrous acid it contains."

I have not used this method, but I hear the results are pretty constant.

"Hart's process," by nitrate of urea, is founded upon the fact that urea, when brought in contact with nitrous acid, is instantly decomposed into carbonic acid and nitrogen, thus:—

$$\text{Urea } C_2 N_2 H_4 O_2 + 2 NO_3 = 2 CO_2 + 4 N + 4 HO,$$

and upon the fact that nitrous acid decomposes iodide of potassium with liberation of free iodine.

If, therefore, a solution of a known quantity of urea be taken and nitrous vitriol dropped into it, so long as any urea remains undecomposed no reaction of nitrous acid is manifested, but so soon as all the

urea is decomposed the nitrous acid produces the well-known reaction upon iodide of potassium, liberates iodine, and in the presence of a solution of starch gives at once the characteristic blue colour of iodide of starch.

The practical part of the process is as follows:—20 grains of nitrate of urea are dissolved in a few ounces of water and the fluid raised to the boil. The nitrous vitriol to be examined is filled into a burette. The solution of urea is removed from the lamp and the nitrous vitriol dropped into it. Brisk effervescence takes place from the evolution of carbonic acid and nitrogen gases; a drop of the fluid is from time to time taken out and added to some drops of solution of iodide of potassium and starch spotted on a white plate. At first the iodine and starch fluid manifest no reaction, but as fresh additions of nitrous vitriol are made to the solution of urea a point is arrived at when the nitrous acid has decomposed all the urea, any further quantity of nitrous acid now remains in the fluid, and a drop of the fluid now added to the spots of iodide and starch developes the colour and indicates the conclusion of the reaction.

The number of graduations of nitrous sulphuric acid used in decomposing the 20 grains of nitrate of urea, indicates at once by a simple calculation, the quantity of nitrous acid present.

The calculation is based on the fact, that for every 20 grains of nitrate of urea there would be required 12·35 grains of nitrous acid, which is equivalent to 27·64 grains of nitrate of soda.

I have made some enquiries among chemists here, but I cannot find any one who has succeeded in making it a practical process.

I have tried it myself on the same sample of nitrous vitriol, four times over, but I cannot say that I have been successful, as will be seen by the following results:—

20 grs.	nitrate of urea	required	334·90	grs.	of nitrous sulphuric acid.
20	„	„	190·30	„	„
20	„	„	187·80	„	„
20	„	„	327·20	„	„

It is possible that I may not have complied with some necessary

condition, but I think I have followed the process exactly as laid down in "Muspratt's Chemistry," Vol. II., page 1040.

There appears to me to be a defect in the process, inasmuch as the reaction is ordered to be effected at a boiling heat.

Now, on adding nitrous vitriol to a boiling solution of nitrate of urea, a very tumultuous effervescence takes place, and, I think, an error of loss of nitrous acid occurs, and from some cause, which I confess I cannot clearly understand, the reaction of iodine does not manifest itself so soon in some cases as in others, the result being that you cannot hit the mark on two occasions successively. There is a further objection, that the heating of the fluid requires time, and where many samples are to be tested, this becomes an object of some importance.

To obviate these difficulties, I have contrived a plan founded upon this reaction of nitrous acid on urea, which is exceedingly simple and gives very constant results.

I have already shown that in the decomposition of urea by nitrous acid there are formed 2 eq. of carbonic acid, 4 eq. of nitrogen, and 4 eq. of water. The carbonic acid and nitrogen escape with effervescence, and by suitable appliances may be weighed, the loss of weight will express, by a simple calculation, the quantity of nitrous acid present.

The apparatus required is an ordinary carbonic acid apparatus, in which the moisture is arrested by sulphuric acid, and by which the acid to be tested can be run into the solution of nitrate of urea in a very gentle stream. Such an instrument is described in Fresenius as a Geissler's carbonic acid apparatus. The one I have been using is on the same principle, but slightly modified in details.

The operations are as follows. The bottle of the instrument is half filled with water, containing about 20 grains of urea or 30 grains nitrate of urea in solution. The instrument is now weighed; the acid tube is filled with the nitrous vitriol, to be tested and weighed again; the difference is the quantity of substance taken.

The tap containing the nitrous vitriol is turned on, and a slow stream of acid falls down through the solution of nitrate of urea to

the bottom of the vessel. In the course of a few minutes the acid tube has become empty, the apparatus is shaken to cause complete admixture, and after the violent effervescence which at first takes place, is over, the instrument is heated to about 200° to separate all the dissolved gas.

When cold, it is sucked through to remove the gas remaining in the apparatus and weighed.

The difference of weight, before and after the operation, indicates the quantity of carbonic acid and nitrogen liberated.

The quantity of nitrous vitriol taken is about 300 grains.

The calculation is as follows:—

$C_2\ N_2\ H_4\ O_2 + 2\ NO_3 = 2\ CO_2 + 4\ N + 4\ HO$; therefore,

$2\ CO_2 + 4\ N$: $2\ NO_3$ quantity of $CO_2 + N$ found,

Equiv. = 100 : 76 : X :: nitrous acid found.

The results are very constant, as will be seen by the following analysis of samples of nitrous vitriol.

		Exp. 1.	Exp. 2.
1st trial by	Dr. Hess,	3·02	2·62
2nd „	do.	3·08	2·60
3rd „	W. Crowder,	3·16	2·63
4th „		2·35	

This process seems to me much preferable to the mode of operating with iodide of potassium and starch, and since it is dependent upon a decomposition which is perfectly definite and constant, and the results of which can be most accurately weighed, I am inclined to believe that it is much more to be depended upon than any process in use at present.

I was anxious to institute a comparison between this method and some process dependent upon the oxidation of nitrous acid into hyponitric or nitric acid. Marguerite's process for oxidizing proto into peroxide of iron, suggested itself as one of the best, from the remarkable distinctness with which the point of complete oxidation is indicated; and the rapidity with which it can be performed was a further recommendation.

With this view, I prepared a solution of permanganate of potash in water, of such strength that a burette divided into 1000 grain measures of water, oxidized a solution containing about 5 grains of iron to peroxide as in Marguerite's method. Now, to peroxidize protoxide of iron, requires the addition of 1 eq. of oxygen to every 2 eq. of iron.

Therefore, if 2 eq. Fe : 1 eq. O :: Fe : O.
56 : 8 :: 5 : 0·714.

In the reaction which takes place when permanganate of potash is added to nitrous vitriol, I assume that the whole of the nitro compounds are there in the form of NO_3. This is in accordance with the experience of those who have paid any attention to the subject, and I further assume that this NO_3 in the addition of permanganate of potash is converted into nitric and not hyponitric acid.

The above 0·714 gr. of oxygen would, therefore, represent the following quantity of NO_3:—

2 eq. O : NO_3 :: O : NO_3
16 : 38 :: 0·714 : 1·70

The practical process is conducted as follows. About 100 to 150 grains of the nitrous sulphuric acid to be tested is weighed out in a beaker of some 6 oz. capacity. The standard solution of permanganate is poured from the burette, and the number of graduations noted at the point at which the vitriol ceases to decolorise the permanganate.

Care must be taken, at the commencement of the operation, not to shake the acid and solution, and to keep a little permanganate floating on top of the vitriol, until the vitriol is somewhat diluted, otherwise there is fear of losing a small quantity of nitrous acid by effervescence before it becomes oxidized; this can, however, be very easily managed with ordinary care. The results are perfectly constant, as may be seen by inspection of the tables.

The operation may be varied with advantage for manufacturing purposes by taking a measured quantity of standard solution of permanganate, and dropping into it from a graduated tube, with a

pinch cock, the vitriol to be tested; and it has this great advantage, that it shortens the time, and being done by measure, it involves no trouble or time in weighing.

At the time when I first made these experiments I was not aware that such a plan (although obvious enough) was in use in any laboratory; but on communicating with Mr. Clapham, of the Walker Alkali Works, I found that he had been using this method very successfully for a long time.

From some cause, which I am as yet quite unable to account for, I find that the results obtained by this process are about one-third less than those obtained by the urea method. Whether there is any inaccuracy in the way of calculating and comparing the results, I cannot yet discover; but by taking a vitriol containing a known quantity of nitrous acid, as determined by the "urea" process, and standardizing the permanganate by that quantity, results are obtained which agree perfectly well with the quantity of NO_3 in other samples as determined by the urea process. It is my intention, as time and opportunity presents itself, to continue the investigation of this very interesting subject of enquiry.

Although a subject of great theoretical interest it is one which is of considerable practical importance, inasmuch as an accurate method of determining nitrous acid cannot fail to assist a manager in settling many questions of practice about which there is at present great difference of opinion. Need I further point out the great advantage that the man who works by "rule of three" has over the man who works by "rule of thumb?"

The annexed tables are the result of a series of experiments in illustration—

1st. Of the constancy of results obtained by the processes adopted.

2nd. A series of analyses of samples of nitrous and denitrated vitriol from four different works, illustrating the three different methods of denitrating, viz.,—1st, by steam alone; 2nd, by a mixture of steam and sulphurous acid; and 3rd, by passing nitrous vitriol down a column, and denitrating it by the hot sulphurous acid as it comes from the burners, which also concentrates the acid at the same time.

ANALYSIS OF VARIOUS SAMPLES OF NITROUS VITRIOL BY THE UREA AND PERMANGANATE PROCESSES, OBTAINED FROM FOUR ACID WORKS ON THE TYNE.

First Works—Acid Denitrated by Steam alone.

	Sp. gr.	By the Urea Process.	By the Permanganate Process.
Acid condensed in burner pipes before entering the chambers ...	150° Tw.	2·03	
Nitrous acid from the cisterns ...	148° Tw.	...	1·04
Ditto ...	do.	...	1·62
Ditto ...	do.	...	1·05
Ditto ...	do.	...	0·83
Ditto ...	do.	...	0·82
Ditto as it ran into denitrators (No. 1 set)	150° Tw.	...	0·98
Ditto as it ran out after denitration by steam	132° Tw.	...	0·16
Ditto as it ran into denitrators (No. 2 set)	150° Tw.	...	1·12
Ditto as it ran out after denitration by steam	126° Tw.	...	0·33
Ditto collected as it flows from absorbing columns (No. 1) ...	148° Tw.	...	1·16
Ditto (No. 2) ...	148° Tw.	...	1·03
Ditto from the concentrating pans	150	...	0·14

Experiments Showing the Absorption which takes place in Passing through Columns.

Total height of absorbing columns 22 feet.

			Sp. gr.	By Urea Process.	Absorption.
1st Exp.	1	Nitrous strength of acid poured into column ...	150° Tw.	0·212	1·733
		Ditto as it ran out ...	...	1·945	
	2	Nitrous strength of acid poured into column ...	...	1·170	0·593
		Ditto as it ran out ...	...	1·763	
	3	Nitrous strength of acid poured into column ...	...	1·124	1·011
		Ditto as it ran out ...	...	2·135	

			Sp. gr.	By Urea Process.	Absorption.
2nd Exp.	1	Nitrous strength of acid poured into column ...	150° Tw.	1·033	0·730
		Ditto as it ran out ...	...	1·763	
	2	Nitrous strength of acid poured into column ...	...	1·778	1·312
		Ditto as it ran out ...	...	2·090	
	3	Nitrous strength of acid poured into column ...	...	2·254	0·248
		Ditto as it ran out ...	...	[2·492	

SECOND WORKS—ACID DENITRATED BY STEAM ALONE.

Nitrate and denitrated acid.

Total height of absorbing columns, 45 feet.

	Sp. gr.	By the Urea process.	Repeated.	Repeated.	Repeated.
No. 1. From the concentrating pans	151 Tw.	0.18			
No. 2. Ditto, after passing through the columns, analysis performed by Dr. Hess	148 Tw.	3·02	3·08		
The same sample, analysis performed by W. Crowder ...		3·16			
No. 3. Another sample, by urea process, after passing through the column	149° Tw.	2·62	2·60	2·63	2·35
The same, by permanganate standardized by a sample of nitrous vitriol, tested by urea process		2·73	2·73		
Result in NO_3 calculated by amount of oxidation ...			1·66		
No. 4. Another sample, tested permanganate as before ...	149° Tw.	2·81	2·84		
Result in NO_3 by oxidation ...			1·77		

THIRD WORKS—ACID DENITRATED BY A MIXTURE OF STEAM AND SULPHUROUS ACID.

Nitrated and denitrated acid. Height of absorbing column, 35 feet.

	Tested with Permanganate.
No. 1. Acid from absorbing columns— Sp. gr. 143° Tw., by oxidation process...	0·444
No. 2. Denitrated acid (by steam and SO_2) Sp. gr. 123° Tw. ...	0·044

		Tested with Permanganate.
No. 3.	Acid from absorbing columns ... ... Sp. gr. 142° Tw.	0·429
No. 4.	Denitrated acid (by steam and SO_2) ... Sp. gr. 121° Tw.	0·026

FOURTH WORKS—ACID DENITRATED BY SULPHUROUS ACID.

Nitrated and denitrated acid.

			Sp. gr.	By Permanganate.	Result in NO_2 calculated by amount of Oxidation. Repeated.
1st Exp.	No. 1.	Denitrated acid	153° Tw. ...	0·15 % ...	0·17
	No. 2.	Acid from absorbing columns	151° Tw. ...	0·96 ...	0·92
	No. 3.	Ditto ditto	152° Tw. ...	0·58 ...	0·63

		Sp. gr.	By Urea Calculation.	Calculated by Oxidation.
No. 1.	Denitrated acid	153° Tw. ...	0·27	... 0 164
No. 2.	Acid from absorbing columns	150° Tw. ...	1·37	... 0·836
No. 3.	Ditto ditto	151° Tw. ...	0·76	... 0·461
No. 1 (*a*).	Denitrated acid	153° Tw. ...		... 0·170
No. 2 (*a*).	Acid from absorbing columns	151° Tw. ...		... 0·825
No. 3 (*a*).	Ditto ditto	152° Tw. ...		... 0·452

Mr. CLAPHAM—I experimented in this direction some time ago. I always found both Hart's urea and the permanganate processes give results above the truth, when compared with the actual quantity passed through the columns.

The PRESIDENT—I have long used a process depending on the addition of the nitrous vitriol to a weak solution of bleach until the latter ceases to bleach indigo.

Dr. C. R. A. WRIGHT read:—

ON THE SPECIFIC GRAVITY OF AQUEOUS HYDRIODIC ACID.

A SHORT time ago a note on the density of aqueous solutions of hydrobromic acid was communicated by the writer to this society; it appeared to be of interest to determine also the relation between

the specific gravity and strength of hydriodic acid, so as to compare the three curves got with these two acids and also with hydrochloric acid.

Diluted hydriodic acid was got, in the first instance, by the action of sulphuretted hydrogen on iodine in presence of water; in this acid iodine was dissolved and the whole digested at a very gentle heat with phosphorus; finally the colourless liquid was distilled twice or thrice successively, the observations being made with the fresh distillate containing no traces of free iodine. The strength was found by a standard alkaline solution, and the specific gravity by weighing a known volume. The results, which only pretend to a moderate degree of accuracy, are as follow :—

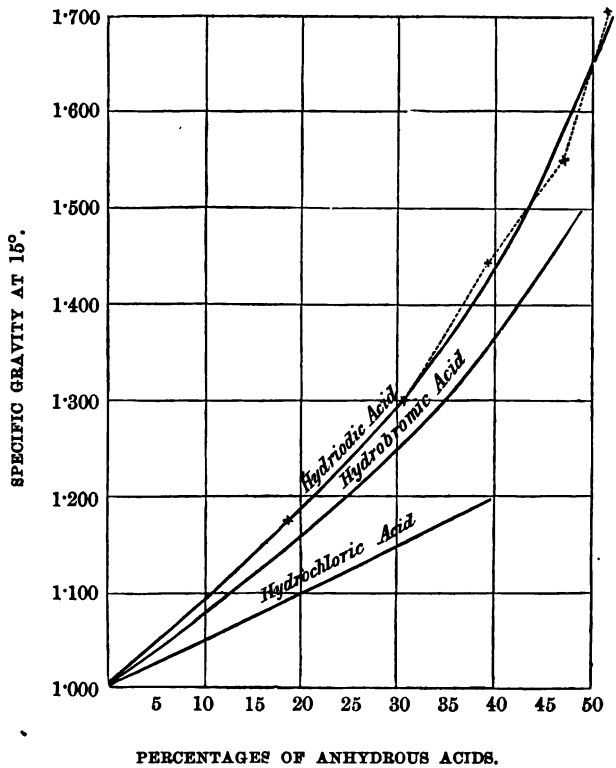
Per Centage of HI.	Sp. Gr. at 15° C.
51·9	1·708
47·2	1·551
39·2	1·442
30·3	1·297
18·5	1·175
5·9	1·053

By graphical representation these numbers are found to agree tolerably together, yielding a *mean curve*, from which the following table is constructed :—

Per Centage of HI.	Sp. Gr. at 15°.	Difference for 5 per Cent.	Difference for 1 per Cent.
0	1·000	.....	.....
5	1·045	0·045	0·0090
10	1·091	0·046	0·0092
15	1·138	0·047	0·0094
20	1·187	0·049	0·0098
25	1·239	0·052	0·0104
30	1·296	0·057	0·0114
35	1·361	0·065	0·0130
40	1·438	0·077	0·0154
45	1·533	0·095	0·0190
50	1·650	0·117	0·0234
52	1·700	.....	0·0250

From these numbers it appears, firstly, that the increase in sp. gr., as with hydrobromic acid, is not proportionate to the increase in strength; and, secondly, that the density of aqueous acid containing a given percentage, x, of HI is, as might be expected, greater than that of aqueous hydrobromic acid containing x percent. of HBr; this, again, being greater than that of hydrochloric acid containing x percent. of HCl.

The annexed diagram illustrates the three curves for the three acids, that of hydrochloric acid being calculated from Dr. Ure's



The dotted line is that given for hydriodic acid by the experimental numbers, the others the "mean curves."

table. The only relationship traceable between the numerical values of the abscissæ and ordinates of these three respective curves is the following approximate one. Let C, B, I be the respective sp. grs. of hydrochloric, hydrobromic, and hydriodic acids containing the same percentage, x, of HCl, HBr, and HI respectively: then B is always greater than the mean of C and I by about 12 percent. of the excess of this mean above the sp. gr. of water (= 1·000) or in symbols:—

$$B = \frac{C + I}{2} + \frac{12}{100} \times \left(\frac{C + I}{2} - 1\right)$$

$$\text{or } B = (C + I) \times 0{\cdot}56 - 0{\cdot}12$$

The following table shows, comparatively, the sp. grs. of the three acids of different strengths, along with the values for hydrobromic acid calculated from the preceding formula:—

Percentage of Anhydrous Acid.	SPECIFIC GRAVITY.			
	Hydrochloric.	Hydriodic.	Hydrobromic (found).	Hydrobromic (calculated).
5	1·0244	1·045	1·038	1·039
10	1·0488	1·091	1·077	1·078
15	1·0734	1·138	1·117	1·119
20	1·0981	1·187	1·159	1·159
25	1·1233	1·239	1·204	1·203
30	1·1484	1·296	1·252	1·249
35	1·1738	1·361	1·305	1·300
40	1·1966	1·438	1·365	1·356
45	.....	1·533	1·435	.....
50	.....	1·650	1·515	.....

It might, perhaps, be expected that the percentages of anhydrous

acids, for the same sp. gr., would be in the inverse proportion of the atomic weights of the anhydrous acids, viz. :—

$$HCl = \frac{1}{36{\cdot}5} \qquad HBr = \frac{1}{81} \qquad HI = \frac{1}{128}$$

or taking the atomic weight of HCl as under :—

$$HCl = 1 \qquad HBr = 0{\cdot}450 \qquad HI = 0{\cdot}285$$

this, however, is not the case : thus the sp. gr. 1·050 corresponds to 10·2 percent. HCl, 6·5 percent. HBr, and 5·5 percent. HI, or HCl = 1, HBr = 0·64, HI = 0·54.

Similarly the sp. grs. 1·100, 1·150, 1·200 give the following ratios :—

Sp. Gr.		Percentage of HCl.		Relative Percentage of HBr.		Relative Percentage of HI.
1·050	...	1·000	...	0·64	...	0·54
1·100	...	1·000	...	0·63	...	0·54
1·150	...	1·000	...	0·62	...	0·53
1·200	...	1·000	...	0·60	...	0·52

The preceding experiments were performed in the laboratory of Messrs. Bell Brothers, Washington.

Messrs. W. W. Proctor and J. G. Richardson, having been elected scrutineers, reported the following as elected for the session, 1871-2 :—

PRESIDENT—John Glover.

VICE-PRESIDENT—I. Lowthian Bell.

TREASURER—John Pattinson.

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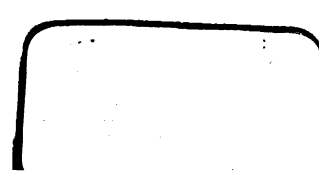
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